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TO

## MARGARET FLOY WASHBURN

TEACHER, EDITOR, AUTHOR, SCIENTIST

Twenty-five years Professor of Psychology at Vassar College and Editor of the American Journal of Psychology

THIS VOLUME IS DEDICATED BY HER COLLEAGUES IN RECOGNITION OF

Thirty-three Years of Distinguished Service to Psychology

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WASHBURN COMMEMORATIVE
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# CONCEPTS AND 'ISMS' IN PSYCHOLOGY

By Joseph Jastrow, University of Wisconsin

The attention now given to the history of science is in every way commendable. The broadening value for every professional scientist of an acquaintance with the intellectual steps by which his science has come to its present status and recognition, is part of the liberal education of the specialist. It is perhaps particularly important in such a discipline as medicine in which the relation between theory and practice is so variable. Such a history is concrete and full of incidents by reason of that relation. The more abstract disciplines lack this realistic touch, and their history consequently follows the line of doctrine. Still further, doctrine arises from the creative contribution of distinctive minds and is likely unduly to carry the impress of personal attitude rather than the objective stamp of inquiry.

In the case of psychology, its late emancipation from the tutelage of philosophy gives its history an irregular perspective. The earlier attempts to project a history of psychology consisted in little more than selecting the psychological portions of the doctrines of representative philosophies; it is on the whole definitely creditable to the importance of psychology that so many of the important philosophical problems followed what we now recognize as psychological clues. As is usual in such affiliations the dominant party claims the credit. Much of what passes as the history of philosophic doctrine is far more directly a chapter in the antecedents of modern psychology. Once psychology was established in the modern sense, the perspective of its problems appeared clearly; we find that the later histories of psychology take their clue from modern interest, and record the step-by-step contributions, often anticipations, of thinkers primarily in-

terested in philosophy. The interest and profit in such an undertaking appears admirably in the masterly study of Stratton on "Physiological Psychology among the Greeks."

As an instance of presenting the distinctive forces that are responsible for the development of psychology, one may cite the division of Dessoir who recognizes the practical interest as "psycosophy" (later applied psychology) parallel with the technical interest in doctrine, which is 'psychology' in its usual accepted meaning. With doctrine once made prominent, the historical path was inevitably that set by the doctrinal survey. We have then such sectional contributions as Warren's of the "History of the Association Psychology;" and we have as equally important the historical presentation of selected problems. Roback's recent book on "Character" contains so large a historical material that it may stand as the model of this procedure. We have there presented the successive stages in our knowledge of this important problem. Similarly Wilm has given us a history of the "Theories of Instinct." I mention this in order to make clear that the history of psychology is not a barren subject—that indeed under the impetus of the modern perspective it has grown in significance.

By way of contrast it is rather noteworthy that the study of philosophy, especially as introduced into the college curriculum, has proceeded so generally in terms of an historical outlook. It was true of many courses in philosophy that they were far more courses in the history of philosophy than in philosophy itself. It would almost appear that philosophers were hard put to define the matter of their considerations, and went eagerly to the history to supply their deficiencies. To center the study of philosophy around its history is hardly a warranted pedagogical device. It is certainly in marked contrast to the procedure in physics and chemistry or biology. Thus with neglect on one side and over-emphasis on the other, the proper placing of the historical aspect is itself an interesting problem.

I find it necessary to present this background for the special consideration that I here have in mind, for I wish to raise the question as to the significant and profitable steps in the evolution of psychology. Owing to the patterns which were followed, that which was made the center of discussion was a certain system of thought, and this frequently took the name of an 'ism.' So marked is the influence of personality that most of the original 'isms' seem to have been personal ones. We have almost forgotten that Platonism was origin-

ally the body of doctrine set forth by Plato. It has come to mean a certain attitude and interest in large human problems. We forget that Epicurianism meant originally a view on a particular problem held by Epicurus. When a movement took another name such as Stoicism (somewhat accidentally from the place from which the doctrine was originated) we soon forgot its origin, and made it represent a distinctive moral quality, quite as much as a philosophical position. The evolution is complete when we turn to such a word as skepticism which is a true name for a doctrine, or electicism, which equally clearly indicates a desire to take the best out of many systems without over-adherence to any one.

The thesis which I venture to maintain is that so far as the interests of modern science, including psychology, go, the whole method of 'isms' is wrongly focused. We suffer from it in the way of confusion and irrelevance and futility of discussion. As a mere label for a particular body of doctrine, it is useful enough. No one would object to associationism as a convenient method of referring to that doctrine; but it clearly represents only a position in regard to the solution of a special problem. It does not mean that an associationist regards every problem in psychology as solved by that method or attitude; if we do we are open to the confusion of the 'ism' fallacy. We have so flagrant a modern example of it that it is at once called to mind. Behaviorism, both as a term and as an 'ism' is an undesirable citizen in the province of psychology. It carries with it just that undesirable implication that if one is a behaviorist, he solves his entire set of psychological problems by this attitude. It is again likely to be 'unionized' and applied to the doctrines of one school or one man, thus concealing the far more important factor that there are all sorts of behaviorism, that in some sense every modern psychologist is a behaviorist, though most of them protest emphatically against the appropriation of that term by one group of thinkers who have no proprietary right to it, and who have spoiled the term for most of its useful service.

If by way of contrast we go back to the most momentous shift of scientific interest of modern times, that inaugurated by the doctrine of evolution, one may see the relations more clearly. Perhaps it is a subject for congratulation that no one called it 'evolutionism,' so deadly is the 'ism' implication. We called it Darwinism so far as it indicated the position of Darwin; but 'evolution' is a concept; it is not an 'ism.' My thesis is that science advances profitably and

significantly in terms of concepts and not in terms of 'isms.' A change in concept, or the introduction of a new concept advances knowledge. Such concepts as evolution, conservation of energy, and such shifts of concept as would be indicated if we should trace the changes in meaning of the term 'instinct,' are the realities that mark the development of science.

When we refer to the theoretical element in a concept we may call it a theory; but concept is still the better name. Thus we have done well to use the term Freudianism, or Freudism, as little as possible. We present his doctrine in terms of the leading concepts such as the subconscious, suppression, conflict, transfer, regression, and so on. Here, unfortunately, the major application of this group of approaches to psychological problems has introduced a false emphasis. Quite too commonly 'psychoanalysis' has usurped the place which belongs to the total group of concepts which have done so much to quicken psychological interest. That they have done so from the study of the abnormal is itself an example of the importance of that concept, which may be stated as the position that normal mental behavior is to be illuminated by many of the concepts that arise out of the study of the abnormal. It would be as unfortunate as misleading if this notion were called 'abnormalism.' So, similarly, the illumination of the entire mental domain from the genetic approach is one of the fertile concepts of modern psychology. Though without sharp boundaries the introduction of the genetic view functions as a concept. It indicates the profitable lines of advance.

Equally characteristic are the shifts in the meaning of concept with the change of name. Such is the concept of 'intelligence,' its rich addition of connotation when it was brought in line with the concept of arrest, thus combining the genetic concept of stages of growth with the increasing complexity of logical processes; all this establishes the value of the concept.

I may pass with a brief mention the most unfortunate tendency of the 'isms' in psychology or elsewhere, namely that when that direction of adherence is followed, it results not in a science, but in a cult. Here the error is flagrant and commonly understood. Cults are devoted to 'isms.' Too many represent a peculiarity of an individual, as when Fletcherism was the exaggerated adherence of Mr. Fletcher to excessive mastication in the maintenance of health. It was equally clear that homeopathy, which began with the peculiar notions of Hahnemann, soon became a cult, deriving its name from its

doctrine *homeopathy*, or like cures like. Science has remained scientific because it has avoided the extreme danger of cults. The 'ism' is the clear invitation and temptation of the cultist.

In the present contribution I have set myself the simple task of calling attention to what seems to me a very important distinction. It is peculiarly important in psychology that we should avoid the emphasis on 'isms' and develop the importance of concepts. The reason why the history of philosophy is so full of futile controversies is that it is divided into schools, which are institutions for the propaganda of a cult, in many cases a legitimate body of adherents to a doctrine. But the irrelevance of 'schools' appears when we consider that they do occupy the same field or share the same interests. There is no alternative or choice between them, for frequently they have no prominent common element. This brings me to my concluding point; namely, that the history of science must take account of the shift of interest, as well as of theory and fact and method. Many doctrines formerly held have not been disproved, but just outgrown—discarded with the wane of interest. We have simply moved cultural miles away from any interest in them. We all know the stock illustration of 'scholasticism'—the discussion of the problem of how many angels can dance on the end of a needle. But there is no greater incongruity of terms in this than in any other problem which has passed the way of all knowledge, and ceased to interest when once the newer concepts have prepared the way for a different perspective as well as a new enlightenment. There have been revolutions and reformations in doctrines; and these have always followed the reconstruction of values, reshaping of interests, so that the old problems lost not what validity they may have had, but lost any hold on the actual intellectual interests of the day. 'Modernism,' if we must call it such, represents a perspective of interest, a mode of approach, as well as access to a substantial body of doctrine.

My plea is accordingly that in the pursuit of psychology we drop the outgrown and barren interest in 'isms,' and substitute the living and profitable interest in concepts. We need this in psychology more than in the other sciences for many reasons. First, because its procedure is less well established, and secondly because the laboratory or investigative method is new, and still presents not merely an advance in method, but a choice of problems to which that method may be profitably applied. Furthermore, we are under the temptation of pushing a profitable idea to an unprofitable extent. There is no

question that the concept of a 'glandular psychology' is legitimate for the body of facts which show that glands exert a vital influence on behavior. We can secure its value by incorporating it into the concept of behavior. But if we attempt to explain the entire differences of personality from that approach, we may end in a glandular cult. Once more, we still are under the influence of 'schools.' We like to classify and place people in terms of their doctrines and to the neglect of their interests. The history of psychology that is to be written, would, as I see it, follow the suggestive line of concepts, and indicate how from age to age as interest changed, as knowledge advanced, newer concepts were needed, would express and give point to the shift of doctrines, to the growth of insight. The historical approach offers an interesting opportunity to set in order the right relation of concepts and 'isms.' But the distinction is equally important in the guidance of the profitable developments of the future. There are many signs at hand in science in general and in psychology in particular, that we are ready for this correction of the fundamentals of our science.

# THE AMBIGUOUS CONCEPT: MEANING

By Mary Whiton Calkins, Wellesley College

I write in the hope of persuading here and there a psychologist, at present addicted to the use of the term 'meaning,' to expel from his professional vocabulary this highly ambiguous, unnecessary, and often misleading word.[1] No technical term is more variously employed and the first task which I set myself is that of distinguishing and estimating these divergent conceptions. There are at least seven of them and they fall into two main groups. In the first of these 'meaning' is conceived as unanalyzable, in the second, as analyzable experience. Under the first head, the conception of meaning as awareness of objects may be distinguished from the vaguer conception of meaning as a special sort of elemental experience; under the second head, four different conceptions of meaning as cognitional are marked off from that of meaning as volitional consciousness. These four cognitional uses comprise the conceptions of meaning as supplemental imagining; as relational experience of any type; as signification; and finally, as implication. The list which follows includes these divisions. It distinguishes

MEANING, As

(I) *Unanalyzable Experience*
- Awareness of objects (*1*)
- Unique, elemental experience (*2*)

(II) *Analyzable Experience*
- Cognitional
  - Supplemental imagination (context) (*3*)
  - Relational experience,
    - Any relational experience (*4*)
    - Signification (*5*)
    - Implication (*6*)
- Volitional experience (purposing and affirming) (*7*)

Under each of these heads a distinction of another sort must next be made: that between subjective and objective meaning. When, for example, Ogden and Richards name both volition and emotion

[1] This paper was delivered, substantially as printed, as one of a series of two lectures on "Conceptions of Meaning and of Value," at the University of London, December 7, 1927.

as "main definitions of meaning,"[2] and when Judd says definitely that meaning is an "attitude of mind,"[3] all three are evidently using 'meaning' in the subjective fashion. But 'to mean' is obviously a transitive verb; correlated with one's meaning-consciousness is always the something-which-one-means; and this something is often referred to as one's 'meaning.' So, when Ogden and Richards state as another sense of meaning "that to which the user of a symbol actually refers,"[4] or when a teacher says to his German pupil, "The meaning of the words 'my knife' is this sharp-edged steel implement," they are illustrating this objective use of the term. Schiller's statement that meaning is "an attitude taken up toward objects by a subject and projected into them"[5] seems to combine both uses. 'Objective,' it should be remarked, is not here used in the sense of 'non-mental.' In other words, one's meaning in the objective sense of what-one-means may as well be psychical as physical. The meaning of the word 'mind,' for example, as truly as that of the word 'body,' is object of my awareness. And 'subjective' meaning, it should at the same time be noted, is conceived in one of two very different ways—either (a) as attitude, or activity, or function of a self (a mind), or else (b) as a mental item or event, simple or complex. For that matter, every experience, not merely 'meaning,' is treated in one of these two fashions.

From this brief consideration of the subjective and the objective aspects of meaning, whatever the specific use of the term, we turn to the discussion of these seven divergent conceptions.

(*1*) The use of 'meaning' in the large sense of 'awareness of objects' is well illustrated both by Titchener and by McDougall, though they estimate it very differently. "All perceptions and ideas," Titchener says, "mean something; a perception is always the perception of a tree . . . or what not."[6] "We can not," McDougall says, to the same effect, "describe meaning otherwise than in terms of the object that we mean; we can only say 'I mean this or that

[2]C. K. Ogden and Y. A. Richards, *The Meaning of Meaning*, 1923, chap. IX, 306 f.

[3]C. H. Judd, *Psychology, General Introduction*, 1907, 267 f. He adds that this attitude "may or may not be filled out with imagery." On the necessity of distinguishing the subjective from the objective aspect of meaning, cf. R. B. Perry, *General Theory of Value*, 1926, 327.

[4]*Op. cit.*, 307.

[5]F. C. S. Schiller, The meaning of 'meaning,' *Mind*, N. S., 29, 1920, 389.

[6]E. B. Titchener, *A Beginner's Psychology*, 1915, 113.

object.' "[7] And a little later on he identifies the "total meaning" of an orange with "the total knowledge which is operative in the mind as I perceive the orange."[8] The two, both Titchener and McDougall, further agree that introspection should not concern itself with meaning. In the view of Titchener, as will presently appear, meaning is mere unanalytical and unscientific consciousness of physical and psychical phenomena. McDougall says explicitly that "meaning cannot itself be made an object of introspective observation as sensory qualities and other modes of experience can, and for this sufficient reason: when we have a physical object we mean it but if we . . . become introspective in the presence of the physical object . . . we find the sensory and imaginal qualities and at that very moment the 'meaning' is changed; we no longer mean the physical object. . .; rather we mean the thinking of the object."[9]

But from their common starting point—that we mean objects, yet can not introspectively analyze our meaning—the two men reach very different conclusions. Titchener instances, as objects of meaning, such physical things as "boats and trains" and such psychical phenomena as "quarrels and successes." These he contrasts with objects of the scientific consciousness such as "masses and velocities" and, on the psychological side, "emotions and volitions." He concludes not only that 'meaning,' thus conceived, is unscientific awareness, but that these objects of meaning, boats and quarrels and the rest, are wholly irrelevant to psychology. In particular he holds that the conscious self, or person, is object, not of justified psychological investigation, but of "moralizing common sense." McDougall, on the other hand, regards meaning, in spite of its unanalyzableness, as an essential factor of experience and ridicules the psychologist who maintains that "psychology must leave meaning to be dealt with by logic"[10] or by ethics. To hold this view, he contends, is to render psychology "a strictly useless science" by "preserving it from all contaminating contact with problems of human life."[11]

As between these precise conclusions one must unhesitatingly, I believe, side with that of McDougall, for Titchener is entirely unjustified in his high-handed rejection of every-day phenomena—

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[7] W. McDougall, *An Outline of Psychology*, 1923, 253.
[8] *Op. cit.*, 256.
[9] *Op. cit.*, 252 f.
[10] Titchener, Prolegomena to the study of introspection, *Amer. J. Psychol.*, 23, 1912, 433 f.
[11] *Op. cit.*, 252.

of boats and quarrels, for example, and in particular, of selves—as objects of scientific experience. Scientific observation differs from unscientific not at all in its objects but in its analytical, classificatory and explanatory method of dealing with these objects.[12] There is, accordingly, no ground for this distinction of 'meaning' from scientific awareness, and therefore no excuse for the exclusion of the sometimes inconvenient objects of the plain man's meaning from the scrutiny of the psychologist.[13] But McDougall has certainly given occasion for this exclusion of 'meaning' from psychology, by his wholly indefensible refusal to analyze the conception. The difficulty which he finds—that introspection changes the object-meant from physical thing to psychic experience—is wholly metaphysical. Meaning, if it is to be a concern of psychology, must be the object of scientific introspection. And such introspection will involve analysis unless 'meaning' is used, as indeed McDougall seems to use it, in so vague a sense as to cover all forms of the awareness of objects. In this wide sense, however, the term loses all distinctiveness, becoming a mere synonym for consciousness or experiencing. Accordingly both these theories of meaning, as unanalyzable awareness of every-day objects, are to be rejected—Titchener's because he forbids the psychologist to study the plain man's objects, and McDougall's because he refuses analytically to introspect the awareness of objects.

(*2*) Akin to these conceptions of meaning as unanalyzable experiencing of objects is that of meaning as 'a unique and unanalyzable relation,' a 'mystical relation between the knower and the known.'[14] This has been called the 'intuition theory,' or the 'mystery theory,' of meaning. William James, though as will later appear he elsewhere most successfully analyzes the concept of meaning, in at least one passage espouses this view, saying that "the sense of our meaning is an entirely peculiar element of the thought," "an absolutely positive sort of feeling," an "evanescent and transitive feeling of tendency."[15]

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[12]Cf. the writer's The self in scientific psychology, *Amer. J. Psychol.*, 26, 1915, 523 f.

[13]Titchener's perversion of the word 'meaning' is the more to be regretted since his consequent taboo of the self (except as physiological organism) has cost psychology the priceless contribution of a master to the scientific study of the plain man's, and the unbiased introspector's, consciousness of self.

[14]Ogden and Richards, *op. cit.*, 384, 306.

[15]William James, *Principles of Psychology*, 1890, I, 472.

This obviously subjective form of the intuition theory of meaning, the conception of it as a unique and unanalyzable way of being conscious, must carefully be distinguished from the objective form of the same theory, often connected with the name of Husserl. "What meaning [die Bedeutung] is," Husserl says, "can be given to us as immediately as color and tone. It cannot be further defined; it is a descriptive ultimate."[16] Husserl and his disciples also teach both that these meanings, as "ideal objects" truly exist since "we apprehend certain truths which refer to them"—as when we say that four is an even number—and that their existence does not depend on their being thought.[17] In other words, they treat the ultimate character of 'meaning' in a philosophically realistic fashion. This, however, is a metaphysical doctrine which the psychologist is not bound to consider.

There is, further, a sound objection to every form, not merely to the realistic form, of this mystery conception of meaning as unique and unanalyzable. For uniqueness and unanalyzableness are not identical characters. The elemental (or unanalyzable) is indeed always unique, but the reverse does not hold; a complex as well as an elemental experience may be unique. Examples are the relational 'feeling of familiarity' and the sensational consciousness of wetness—each a highly distinctive fusion or complex of several elements. Meaning, accordingly, though without doubt a unique experience, is not for this reason elemental; and the psychologist, like every scientist, is bound to suspect the alleged simplicity of any experience and to carry analysis as far as it will go. He must therefore reject the empty mystery conception in favor of any satisfactory doctrine of meaning as further analyzable.[18]

[16]E. G. Husserl, *Logische Untersuchungen*, 1901, 1-ter Theil, 1-tes Kapitel, 181, quoted by Ogden and Richards, *op. cit.*, 423. Cf. Th. Lipps who virtually identifies *das Glauben* and *das Urteilen* with *das Meinen* and then describes *das Urteil*, which is "timeless, without position in space, and impersonal," as the "specific object of logic" (*Psychologische Untersuchungen*, II, 9, quoted by Ogden and Richards, *op. cit.*, 135). Cf. also Bertrand Russell, *Analysis of Mind*, 179 *et al.*

[17]Husserl, *op. cit.*, II, 2-tes Kapitel, 124, quoted by Ogden and Richards, *op. cit.*, 423.

[18]A good illustration of the vagueness and emptiness of this mystery conception of meaning is embodied in Elizabeth Amen's experimental study of Self-awareness and Meaning (*Pyschol. Monog.*, No. 165, 1926). Dr. Amen puts forward, on the basis of the introspective records of her subjects, a doctrine of the consciousness of self as a fusion of meaning with a sensory-imaginal complex. But because of her tacit assumption that meaning is a unique, and further irreducible experience, she leaves completely unanalyzed and vague the rich and varied forms of the consciousness of self. (For a detailed criticism of Miss Amen's monograph, cf. a paper by the writer in *Amer. J. Psychol.*, 38, 1927, 441-448.)

(*3*) We are driven accordingly to the study of other doctrines of meaning and may well begin with the cognitional conceptions. First among these is the theory that meaning consists in the associated imaginal context of any experience. According to this view, the meaning of a boy's perception of a lawn-mower is the visual image of a well-mown lawn; the meaning of a music-lover's percept of a five-dollar bill is an image of the opera house; and the meaning of the verbal percept *Katze* is the image of a long-tailed, furry, little animal. In Titchener's words: "An idea means another idea, is psychologically the meaning of that other idea, if it is that idea's context." And by 'context' most upholders of this view mean supplemental imagery.[19]

We have first to note, in comment on this theory, that it certainly embodies part at least of the historic conception of meaning. It would be difficult, if not impossible, to find an example of a meaning which does not involve supplemental imagery. On the other hand, the bare occurrence of supplemental imagery cannot be held to constitute meaning, unless one deliberately disregards the ordinary use of the term. On this point there is wide agreement among students of meaning. Ogden and Richards assert unequivocally that images

[19]Titchener, *Experimental Pyschology of the Thought Processes*, 1909, 175 f. Cf. his *Textbook of Psychology*, 1910, 367. This, the most emphasized and reiterated of Titchener's doctrines of meaning, seems to be wholly out of relation with his account, already outlined, of meaning as unscientific awareness of objects. I am both leaving out of account a very opaque distinction, which Titchener makes in the passage just cited (*Textbook*, 367) between context, conceived as 'fringe,' and supplemental imagery, and I am also ignoring a hypothesis which he puts forward, so far as I know alone among psychologists, concerning the *primitive* conception of meaning. "Meaning," he says, at the end of the passage here quoted, "is originally kinaesthesis: the organism faces the situation by some bodily attitude and the characteristic sensations which the attitude arouses . . . are psychologically the meaning of that process." Afterwards "meaning may be mainly a matter of sensations of the special senses, or of images, or of . . . organic sensations as the . . . situation demands."

From this conception of meaning as bodily attitude supplemented by kinaesthetic consciousness it is but a step to the most extreme of Titchener's doctrines, that of 'unconscious meaning.' He upholds it in such passages as the following: "I doubt if meaning need necessarily be conscious at all—if it may not be carried in purely physiological terms. In rapid reading, . . . in shifting from one language to another, . . . I doubt if meaning necessarily has any kind of conscious representation" (*Experimental Psychology of the Thought Processes*, 178). But this conclusion, in the first place, confuses the overt use of language with meaning, whereas one may pronounce words quite mechanically without meaning anything at all. And, in the second place, the fact that an unconscious physiological process carries, that is, conditions, a meaning, does not prove that the meaning is itself physiological and unconscious. In a word, though no one denies that an organism faces different situations by different bodily attitudes, there seems to be no authority for the foisting of the term 'meaning' upon such attitudes.

play no essential part in meaning.[20] "The fact," Hoernlé says, "that a significant psychic whole contains as a subordinate factor these sensational elements and that such a whole apparently cannot be present without . . . such elements . . . in no wise entitles us to treat them as the only solid and substantial factor" of meaning.[21] Similarly, Schiller observes that "mental images undoubtedly occur and carry meaning," *i.e.* "stand for and mean" but are not "pivotal," do not "possess inherent meanings." And in support of this conclusion he points to the significant fact that vivid imagery and clear meaning are not always correlated.[22] It is easy to convince oneself of this fact. The French word *sage*, for example, which I have just read in one of the columns of a French journal, immediately and vividly suggests to me the following context, or supplemental series of images: the images of a Breton donkey, of his peasant driver, of a rocky headland. The imagery is readily explained by the fact that I once took an afternoon's drive behind an unwilling Breton donkey whose owner and driver repeatedly described him as "un bête bien sage." But donkey-peasant-rocky coast, though a series of supplemental images, do not constitute what either plain man or psychologist would call the meaning of the French word *sage*. And the reason is not far to seek. The imagery, vivid as it is, is peculiar to my experience, whereas meanings are or may be shared experiences. The verbal perception 'donkey,' for example, means a four-footed, long-eared, braying animal, not to me only but to all English-speaking people. Imagery, accordingly, however vivid, since it varies with individual and with environment, cannot constitute meaning, for meaning in this sense is always possibly, if not actually, a social experience.

(*4*) In sharp contrast with this imagery-conception is another, less widely held, which defines meaning as 'thinking' (and 'thought') or, more broadly, as relational consciousness and relation. Bosanquet uses the word in this fashion when he contrasts idea as 'logical meaning' with "the psychical image which passes through the mind;"[23] McDougall seems at times to conceive meaning in the same way.[24]

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[20]*Op. cit.*, 150 f.
[21]R. F. A. Hoernlé, Image, idea and meaning, *Mind*, N. S., 16, 1907, 79.
[22]Schiller, *op. cit.*, *Mind*, N. S., 29, 1920, 393-394.
[23]B. Bosanquet, *Logic, or the Morphology of Knowledge*, 1888, I, 46.
[24]In the following statement by McDougall there is a suggestion of an even vaguer conception of meaning as non-sensational consciousness of any sort: "And this 'meaning' is far richer than any imagery which I may introspectively discover as playing a part in the act of perception" (*op. cit.*, 256).

J. M. Keynes distinguishes "the ideas or meanings which we may be said to understand" from those other "objects of direct acquaintance . . . our own sensations;"[25] and Angell, finally, in his text-books of psychology consistently employs the term in a similar fashion.[26] As forms of meaning he enumerates "the noting of differences and likenesses; recognition; and conception, most overt and elaborate form [of] consciousness of meaning."[27] The use of meaning in this general sense of thinking or relational consciousness seems, however, to diverge too much from popular usage to be expedient. We do not employ 'meaning' as a synonym for 'reasoning' nor do we speak of 'meaning' a friend's face in a crowd or of 'meaning' either or both shades of blue which we compare. In a word, it is, to say the least, inexpedient to use meaning as a synonym for relational consciousness in general.

A curious metaphysical use of 'meaning' in this non-sensational sense should briefly be noted in conclusion. The "critical realists," Strong, Santayana, and others, have laid hold of the term 'meaning,' in the sense of 'concept' or 'universal,' have then proceeded to divorce it from its relation to conceiving and conceiver and have ended by identifying it with their 'essence' or 'datum'—an object of knowledge which is none the less defined as 'independent' of the knowledge process.[28] With this metaphysical monstrosity we, as psychologists, are happily not concerned.

(*5*) The rejection of the view that meaning is or may be a synonym for thinking in general does not, however, discredit any conception of meaning as a specific sort of relational consciousness. There are two such theories, of which the more important conceives meaning as 'signifying' or 'referring to' an object. This is historically the earliest use, as it is preëminently the logician's and the linguist's use, of the word meaning. Furthermore, every writer, in whatever other sense he may use the term, also employs it in this way. Thus Schiller, though he advocates the volitional conception, which will later be discussed, himself uses the phrase 'stand for' as synonym

[25] J. M. Keynes, *A Treatise on Probability*, 1921, Chap. II, 12 f.; cited by Ogden and Richards, 135.

[26] J. R. Angell, *Psychology*, 2nd ed., 1905, Chap. X, 246.

[27] *Op. cit.*, 246-248.

[28] For discussion and criticism of this doctrine, cf. a paper by the writer, On certain difficulties in the modern doctrine of essence, *J. Philos.*, 23, 1926, 701-710. It is a question whether Husserl and his fellows do not often use 'meaning' in this sense of logical, yet non-mental entity, rather than in that of unanalyzable character.

for 'mean.'[29] So also Russell says of "meaning" that it "makes us feel that the image points to an object."[30] Even Titchener, for all his teaching that meaning (when it is not a synonym for 'unscientific awareness') consists in supplementary images or in kinaesthetic sensations, constantly and as a matter of course employs the term in this sense of 'standing for' or 'signifying.'

The discussion of this theory of meaning will constitute a disproportionately long section of this paper. It will naturally fall into two main parts: the study of meaning, or 'referring,' as conscious experience; and the study of the objects of meaning. Under the latter head we must take into account no fewer than three objects-of-meaning.

(1) The first of these is 'the meaning' in the sense, already discussed, of *the object* or the class-of-objects which *is meant*. For this type of object Ogden and Richards propose the useful synonym, 'referent.'[31]

(2) The second of the objects of the meaning-experience is the sign. One is conscious, when meaning, not merely of the object or class of objects which one means but also of the sign by which one means this object. Through this awareness of a sign, whether word, or gesture, or diagram,[32] one refers to (means) the referent, or meant-object. Metaphorically therefore the signs themselves—words, gestures or diagrams—are said to have meaning. The literally correct statement, "By the auditory-kinaesthetic sign p-e-n I mean an implement for writing," is popularly shortened to the statement, "P-e-n means the implement for writing." The usage, too convenient to be yielded, has doubtless contributed to the haziness of the concept of meaning. A later paragraph will elaborate this conception of the sign.

(3) The third of these objects of the meaning-consciousness is the self with whom the signifying, meaning self is in intercourse. For meaning, though conceivably a solitary procedure, is normally social. That is to say, I refer, not merely myself, but my fellow-selves also, through a sign, to a meant object (a referent.) To the discussion of this, the social object of meaning, we shall also later recur.

The preceding paragraphs have indicated the widespread use of the term 'meaning' in the sense of signifying, with special regard to the objects involved. A closer study of this experience of meaning (that is, signifying or referring) through the use of signs, has next to be

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[29]Schiller, *op. cit.*, 393.

[30]B. Russell, *Analysis of Mind*, 179.

[31]*Op. cit.*, 14 *et al.* The term has the advantage of exact correspondence with the phrase 'referring to,' a recognized synonym for subjective meaning.

[32]On the different types of sign, cf. Baldwin's *Dictionary of Philosophy and Psychology*. C. S. Peirce distinguishes between 'icon,' 'index,' and 'symbol;' and G. F. Stout between demonstrative, discriminative, mnemonic, expressive and symbolic signs.

undertaken. The problem involved may best be concretely stated. Precisely what is it, for example, to 'mean' by the word 'hyacinth,' a fragrant flower of a particular complex shape, either blue, white, lavendar, or pink in color? The answer seems to run as follows: To mean by the word 'hyacinth' the concrete object, hyacinth, is to perceive or imagine the word 'hyacinth' with the supplementary but mainly relational consciousness, implicit or explicit, that the essential character of this verbal experience is its function of regularly or habitually suggesting, as object (or referent) the fragrant, delicately-colored flower. The fact that one regularly imagines the object, after perceiving or imagining the sign, is a special case of associative imagining due to the circumstance that in the past the word 'hyacinth' has been spoken, read, or heard in close connection with the consciousness of the flower; but the awareness of a sign as 'standing for' or 'meaning' something, is a relational not an imaginal experience. A very clear account of this doctrine of meaning is offered by James in his chapter on Reasoning. In characteristic unconcern for his earlier avowal of the intuitional theory of meaning, James here discusses "the notion of a sign as such" and, referring to the different sorts of bark by which a dog communicates with his master, says unambiguously: "The 'yelp,' the 'beg,' the 'rat,' differ as to their several imports and natures. They agree only in so far as they have the same *use* to *be signs*, to stand for something more important than themselves."[33] By way of illustration, James refers to Samuel Howe's account of his training of the blind-deaf-mute, Laura Bridgman. "He began," says James, "by gumming raised letters on various familiar objects. The child was taught . . . to pick out a certain number of particular articles when made to feel the letters. But this was merely a collection of particular signs, out of the mass of which the general purpose of *signification* had not yet been extracted by the child's mind. Dr. Howe compares his situation at this moment to that of one lowering a line to the bottom of the deep sea in which Laura's soul lay and waiting until she should spontaneously take hold of it. . . . The moment came 'accompanied by a radiant flash of intelligence and glow of joy;' she seemed suddenly to become aware of the general purpose imbedded in the different details of all these signs and from that

[33] James, *Principles of Psychology*, II, 357.

moment her education went on with extreme rapidity."[34] Ogden and Richards formulate what is virtually the same conception though they camouflage it as a causal theory.[35]

It will at once occur to the attentive reader that this conception of meaning as signification combines the important features of all save one of the theories rejected in the preceding pages of this paper. For, in terms of this theory, meaning is evidently a certain unique sort of awareness of objects involving both a relational consciousness and (in most if not all cases)[36] an imaginal context. Yet this signification-concept of meaning is as evidently distinct from any one of the others. For it conceives meaning as analyzable though unique; it identifies meaning not with awareness of every sort, but merely with the signifying of objects; it counts as meaning not any and all relational experience, but only the relational consciousness of 'referring,' and, finally, it holds that imaginal context so far from constituting meaning is a constantly varying and even a sometimes lacking accompaniment or ingredient of meaning. This last point needs emphasis because meaning, as signification, is so often identified with imaginal context. It cannot, therefore, too often be reiterated that not all imaginal context is meaning. When, for example, the first glimpse of a columned porch suggests a Greek temple, one can not rightly be said to 'mean' the temple by the porch nor can the perceived porch be described as the 'sign' of the imaged temple. For only an object realized as uniformly suggesting the temple can be held to be sign of it, whereas my glance at the porch is quite as likely to be followed by my imaging a hammock, a rambler rose, or a postman, as by my imaging a temple. On the other hand, hearing, speaking, or reading the word 'temple' is regularly followed[37] by my ment-

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[34] *Ibid.*, 358.

[35] "A sign," these writers say, "is always a stimulus, similar to some part of an original stimulus and sufficient to call up . . . an excitation (similar to that caused by the original stimulus)" which, in turn, conditions a "mental process." *Op. cit.*, 139-140, with footnote. The conception is 'causal' however, only when it is stated in the artificial terms of sequent ideas. Indeed, Ogden and Richards themselves admit that the treatment of the theory as causal is "convenient merely for the sake of its brevity." *Op. cit.*, 137-138.

They further designate their doctrine as a context-theory, but mean by 'context' more than supplementary imagery. *Op. cit.*, 145.

[36] The exceptional cases, if any, are those in which a word such as 'if,' or 'but,' or again, such as 'pleasure,' stands for a non-sensational experience. But it would be beside the main purpose of this paper to raise the old imageless-consciousness issue here involved.

[37] For "regularly," Ogden and Richards substitute, "nearly uniformly."

ally picturing fluted columns, triglyphs, metopes and pediments. The reason for this uniformity of the sign's suggestiveness has already been indicated. The nature of the object which I regard as a sign is in fact precisely this, that it has no essential concrete character of its own, no essential quality, save precisely this, that of 'regularly suggesting' or 'standing for' another object. This is to say, that, when I perceive the 'sign,' I am ordinarily and normally aware of another definite object more important than the sign. The perceived porch, for example, is so rich in its content of form and color that my attention fastens now on one feature of it and again on another; and my perceiving of it is followed now by this and again by that other imagining. On the other hand, when I hear or catch sight of the word t-e-m-p-l-u-m, I perceive an object which is poor in suggestion and possessed indeed of concrete characters so unimportant in themselves that quite different ones may be substituted for them, for example, those of the word *ναός*. The essential character of either word is simply that of being the sign of the concrete object, temple; in subjective terms, I mean the temple by the word.

It is high time to recur to the consideration of intercourse as implied in meaning. That intercourse, or communication, is a factor of meaning (when meaning is used for signifying) is indirectly proved by the fact that conventional language, the organized system of verbal signs, has as every one admits, grown up primarily through the realized desire and need for intercourse between persons or selves; it is an expression of instinctive communicativeness. In Dittrich's phrase, "communication is of the essence of language."[38] Otherwise stated, a sign is not merely *sign of* the referent, or object meant, but also *sign for* the fellow-self, the indirect object of meaning. Bosanquet accordingly defines meaning as "that identical reference of symbols by which mutual understanding between rational beings is made possible;"[39] Stout describes demonstrative meaning as the "act by which one individual . . . draws the attention of another individual to an object;"[40] and Royce, in more technical phraseology, conceives interpretation (his term for meaning) as a triadic, asymmetrical, social relation involving as its terms first, the interpreter and his

[38]O. Dittrich, *Die Probleme der Sprachpsychologie*, 1913, 11-12, quoted by Ogden and Richards, *op. cit.*, 367.

[39]Bosanquet, *op. cit.*, 4.

[40]Cf. Baldwin's *Dictionary of Philosophy and Psychology*, article "Sign-making Function."

interpreting, second, the interpreted, and third, the person interpreted to.[41] Ogden and Richards, also, among recent writers, stress this social aspect of meaning,[42] from the standpoint of their special interest, the nature and function of language. "Under symbolic function," they say, "are included both the symbolization of reference and its communication to the listener, *i.e.* the causing in the listener of a similar reference."[43] A psychologically, logically, and empirically important distinction between objects-meant is based upon this relation of interpreter to person-interpreted-to or, more simply, on the relation of speaker to listener. This is the difference between the objects-meant by the speaker (the 'user of signs') and the objects which the listener believes the speaker to mean. Much of the tragedy, the comedy, and the confusion of life rises out of the difference between these two sets of meanings.

The unbiased reader will not fail to notice that these distinctions in social meaning can be conceived only in terms of self psychology, whereas meaning as mere reference of sign to object is compactly, though inadequately, stateable in the terms of an impersonalistic introspective psychology, that is, as habitual succession of one mental event, or 'process,' on another. But that one mental event, or process, or phenomenon, should communicate with another is sheer absurdity. And behavorism is equally impotent to deal with social intercourse. Behavorists, it is true, always claim the right to a social psychology but their assumption of social intercourse inevitably implies what officially behaviorism denies, that the communicating organisms are conscious. Barking and talking, for example, from the strictly behavoristic standpoint, are mere phenomena of sound vibration and bodily response; they constitute communication only as they imply the mutual signifying of conscious beings.

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[41] J. Royce, *The Problem of Christianity*, 1913, II, Lecture XIV.

[42] Cf. *op. cit.*, 136: "All experience is either enjoyed or interpreted (*i.e.* treated as a sign)." The term interpreter seems to be used by these writers as equivalent to Royce's 'self-interpreted-to,' while Royce's interpreter is described by Ogden and Richards as 'user of a symbol,' *op. cit.*, 307.

[43] *Op. cit.*, 258; cf. 344 ff. The highly important distinction admirably elaborated by Ogden and Richards, between the symbolic function (that is, the meaning function) and the emotive function of language, is irrelevant to the present study. (Cf. *op. cit.*, 226, 257, 263, 254, *et al.*) Other irrelevant but significant features of this book (which, despite its title, is a study of language, and only incidentally of meaning) are the distinction between word-dependence and word-freedom (*op. cit.*, 345 ff.) and the conception of definition as substitution for one symbol, of a symbol better understood (*op. cit.*, 387).

(*6*) A single paragraph will serve to state and briefly to illustrate the conception of meaning, in our sixth sense, as implication. It is exemplified by such statements as the following: *Vanity means egoism* and *Forbidding a Socialist meeting on the Boston Common means the withdrawal of the right of free assembly.* In these statements "means" is clearly used in the precise sense of 'implies,' which is to say that meaning is here a synonym for implication.[44] Like the conception just discussed of meaning as signifying, this is a cognitional and relational theory. The difference between the two conceptions must not, however, be obscured. In implying I make use of no sign; and meaning as implication is attributed never to words or gestures, but to concrete objects and situations. It is not, for example, the case that the words "forbidding a meeting," are realized as meaning, or standing for, the factual withdrawal of the right of assembly, but that the factual forbidding is realized as part of the total situation which includes the withdrawal of rights.[45] In truth, 'implying,' though of course a kind of relational consciousness, is yet distinguishable mainly in terms of the relation which constitutes its object. In other words, implication is more significantly the concern of the logician than that of the psychologist. A closer analysis will, therefore, not be undertaken.

(*7*) There remains a final and a psychologically distinctive sense of the term 'meaning.' When I say that I mean to play golf, or to climb Grizzly, or to write letters; or again when I insist that I "mean" what I say in issuing an order, obviously I am not using the word 'meaning' as a synonym either for 'signify' or for 'imply.' These last two are cognitional senses of the term; while 'meaning' to write or to climb is simply 'purposing' or 'intending;' and 'meaning' what I say is 'affirming.' Together, purposing and affirming constitute what may be called the volitional type of meaning.[46] In this volitional sense, anything has meaning from the objective standpoint when it embodies somebody's purpose or affirmation, whereas any-

[44]Cf. Ogden and Richards, *op. cit.*, 307, X.

[45]Cf. *loc. cit.*, VIII.

[46]W. M. Urban (*Valuation and its Laws*, chaps. III-V) and Ogden and Richards (*op. cit.*, 307 ff., 324) recognize the "affective-volitional," in place of the "volitional" conception of meaning. In other words, they take meaning in this use, as precisely synonymous with valuing and value. The writer of this paper holds, on the contrary, that 'meaning' is rarely if ever used precisely in the sense of 'emotion' or of enjoyment. Certainly, the use, if it occurs, characterizes metaphysical, rather than psychological, theory.

thing—a book, a game, a life—is meaningless when it is purposeless. And meaning as volition, in its subjective aspect, is, in Schiller's phrase already quoted, "an activity or attitude taken up towards objects by a subject and . . . projected into them."[47] This subject, or self, which projects into objects its meaning or purpose is, in other words, a voluntary "agent" as contrasted with the mere "spectator" self which is aware of signs as standing for objects. Clearly, this conception of meaning, widely as it differs from the two already admitted, has been sanctioned by constant usage. Evidently also it opposes neither of these cognitional uses but may be combined with either of them. Thus, I may mean to mean in the sense that I intend to refer or to imply.

The emerging conclusions of this paper are then the following: It has been shown that the word 'meaning' is employed in seven main senses, some inconsistent with each other, and all clearly distinguishable. Closer study has disclosed good reasons for rejecting four at least of these conceptions. The conception of meaning as unanalyzable awareness of objects should be abandoned because it involves either (1) the exclusion from psychology of the objects of the plain man's consciousness or else (2) the admission to the world of the psychologist of experience immune from analytical introspection. The intuition theory must be rejected because it fails to prove, in the face of other theories, the impossibility of analyzing the meaning-experience. The imaginal context theory must be yielded because, by common admission, not all supplementary imaginings are meanings. Finally, the relational consciousness conception in its inclusive form must be discounted as artificial and unnecessary.

Three conceptions only have resisted criticism. In other words, the term 'meaning' has been shown to have but three irreproachable meanings: on the one hand, the two cognitional uses of meaning as signification and as implication, and, on the other hand, the use of meaning as volition in its two forms, purpose and affirmation. I urge, however, that in no one of these senses is the concept of 'meaning' both essential to psychology and distinctive of it. In the sense of 'implication,' meaning belongs not to psychology at all but to logic. In the sense of 'signification,' meaning is a term no more

[47] Schiller, *op. cit.*, *Mind*, N. S., 29, 389. In the far less adequate terms of impersonalistic, introspective psychology, subjective meaning is a 'purpose' and 'an affirmation.'

essential to psychology than to the theory of language, to every other science, and to social intercourse; it is in no way the peculiar property of psychology. Only when conceived as 'volition' is meaning a specifically psychological term and here it may well be treated as an untechnical alternative for the more unambiguous words, 'purposing' and 'affirming,' of which it is a synonym. There is thus no decisive reason why psychology should insistently claim the concept of meaning, even in one of these defensible senses. Experience has shown that it is fatally easy to glide from a proper to an illicit use of the term; and certainly it stands to reason that a word used in at least seven definite senses, four of them unjustified, is unfitted for the purposes of effective exposition and discussion. I end accordingly as I began,—by appealing to psychologists to abjure entirely the use of the slippery conception, 'meaning.'

# OUTLINE OF A PSYCHOLOGICAL STANDPOINT

By Howard C. Warren, Princeton University

In previous papers[1] some of the fundamental problems of psychology have been examined—problems concerning the relations of psychology to other fields of science, the character of its data, the legitimacy and value of the methods used in psychological investigation, and the validity of the assumptions made by the several psychological schools. In the present paper the results of these separate discussions will be brought together to give a systematic view of the subject. It is believed that the position taken is self-consistent; that our assumptions are in harmony with the known facts; and that our standpoint is in agreement with the mechanistic world-view held by a large proportion of scientists. The conclusions reached in the earlier papers will in some cases be merely restated with appropriate references. In other cases they will be amplified to meet criticisms and to correct misunderstandings.

## The Field of Psychology

The situation in psychology today is peculiar. Two different groups of investigators, the introspectionists and the behaviorists, are working in substantially the same field; each practically ignores the systematized discoveries of the other, and each claims to represent the only genuine science of psychology. This development is quite contrary to the general course of scientific progress, which is bringing the separate fields of investigation into ever closer relation. Physics and chemistry are no longer treated as wholly distinct. Biochemistry provides a link between these basal sciences and biology. The whole trend of scientific discovery suggests and supports the assumption that Nature constitutes a single interrelated system of phenomena.[2]

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[1] H. C. Warren, The mental and the physical [MP], *Psychol. Rev.*, 21, 1914, 79-100; A study of purpose [SP] *J. Phil.*, 13, 1916, 5-26, 29-49, 57-72; The mechanics of intelligence [MI], *Phil. Rev.*, 26, 1917, 602-621; Psychology and the central nervous system [PNS], *Psychol. Rev.*, 28, 1921, 249-269; Mechanism and teleology in psychology [MTP], *Psychol. Rev.*, 32, 1925, 266-284. Cf. also *Human Psychology* [HP], 1919, Appendix I-III. The abbreviations in brackets are used in the references and citations throughout this paper.

[2] That Nature constitutes a single system of things is treated by many scientists as a postulate. As a *postulate* it may be challenged, but the progress of research lends ever increasing support to it as an *hypothesis*.

What seems needed at present is a definition of psychology which will serve both introspectionist and behaviorist in their researches, and will bring our field as a whole into vital relation with the other natural sciences. Such a definition should be based, not on theoretical considerations, but on the subject matter of investigation.

From the standpoint of general science the branch most closely related to psychology is physiology. Both are concerned with certain activities of the individual organism. The place of psychology in the general scheme of things, then, will be sufficiently determined if we distinguish it from this kindred branch. Introspectionists are accustomed to differentiate the two by the statement that physiology deals with chemical and physical processes within the organism, while psychology deals with the individual's mental processes. Obviously this type of distinction rules out behavior study from psychology; it is also objectionable in that it fails to supply a generic mark whereby both psychology and physiology can be grouped together in a higher class. If the data of psychology belong within the general realm of nature which science investigates, then they should not only bear a distinguishing mark, but they should also possess some character in common with the data of kindred branches.

Both the specific and generic characters, I believe, are to be found, not in the processes which take place within the organism itself, but in the relation which these operations bear to the environment. It has long been recognized that conscious experiences as well as organic behavior are generated by external stimulation. The conscious, reacting individual is dependent upon his surroundings for the beginning of the chain of events which result eventually in conscious experience and motor activity. It is quite as evident that the life processes investigated by physiology are dependent upon the environment. The organism, as a living unit, draws its sustenance, the means of carrying on, from its surroundings. The investigation of the relations between organism and environment, then, may serve as the generic mark of the two sciences.

But the relation between the organism and its environment is essentially different in the two cases. Physiology investigates the operations which are concerned with the maintenance of life, with the production and organization of the material necessary to preserve the organism as a whole, with the processes involved in developing and maintaining its specific organs. Psychology investigates the

processes whereby the organism adjusts itself as a unitary system or thing to its surroundings, either by altering its relations to the environment, or by acting upon the environment.

The fundamental difference between the two sciences, then, seems to rest upon the kind of interplay which takes place between organism and environment. Physiological operations depend upon the taking in of certain chemical substances ('nutriment') and the giving forth of other chemical substances ('waste'). Psychological operations depend upon the interchange between organism and environment of certain forms of activity. In other words, physiology investigates the interchange of *materials*, while psychology investigates the interchange of *activity*. The former studies intake and output; the latter studies impression and expression, that is, communication.[3]

While this contrast between their data indicates the basal distinction between the two sciences, some qualification is necessary. Not every interchange of energy between organism and environment is of psychological import. The action of heat upon the body, for example, is largely physiological. The significance of responsive activity lies in the adjustment which it brings about; and this involves the *selection* of certain elements or conditions in the total situation. The organism selects specific forces (stimuli) from among the great array of forces operating in the outer world; and this selection is made possible by the evolution of specific receptors. It also selects specific modes of organic activity (responses) from among the many possible motor reactions; and this selection is made possible by the evolution of specific effectors. It is the evolution of these organs that in the first place transforms the intercommunication from a random to an adjustive one. More important in the higher stages of organic evolution is the system of transmission-paths within the body, which connects the receptors with the effectors and integrates the responsive activity. The significant points, then, which distinguish psychology from physiology are (1) interchange of activity rather than of substance, (2) selection of specific activity patterns from among the total forces operating in the organism and environment, and (3) evolution of organs for selecting these activity patterns.

We may accordingly define the field of psychology as *the investigation of all the phenomena concerned in the transfer or communica-*

[3]The term *communication* is here employed in a general sense, corresponding to the verb *communicate* as used *e.g.* in physics. It means the transmission of non-material effects.

*tion of selected energy patterns from the environment to the organism and the reactive transfer of energy patterns from organism to environment by means of appropriate organs, together with the intermediate processes of every sort that occur within the organism.*[4]

How does this conception fit in with the theoretical standpoints of the several contemporary schools? The behaviorist tells us that he is investigating responses to stimulation. But stimulation means the transfer, or communication, of selected patterns of activity by the environment to the nervous system[5] of the organism, and ultimately the communication (in return) of selected neural activity to the environment in some pattern of response or expression. Evidently the behaviorist could accept our definition without modification.

The introspective psychologist tells us that he is concerned with the consciousness or experiences of the individual. Actually, these experiences are closely bound up with this same communication of impressions, this transfer of specific physical activity, through stimulation, from the environment to the nervous system. Even though he does not explicitly investigate the retransfer of activity to the environment which constitutes the response, the introspectionist takes it into account indirectly through the kinesthetic sensations which ensue. And whenever he extends his investigations to include the experiences of others, he implicitly makes use of their verbal responses. He, too, might be willing to acquiesce in our definition.

I believe that all schools of contemporary psychology could adopt this general demarcation of the field, though each group would have its own private opinion as to the relative importance of the various stages in the course of events. The radical behaviorist would emphasize the mechanism of reception and response, denying that 'knowledge of the external situation' by the individual, if a fact, is of any scientific significance. The structuralist would emphasize the 'subjective aspect' of the data received, which he calls sensations, perceptions, ideas, etc. The teleologist would insist on the presence of a purposive factor in connection with the central processes, which essentially controls or modifies the nature of the response. These

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[4]This definition is formulated in more general terms than those which I have proposed elsewhere, but it covers the same ground. (Cf. PNS, 251; HP, 13.) The notion of *adjustment* is not included in the definition, since this is a result and not part of the process itself.

[5]Or, more precisely, the conducting system, since unicellular organisms and other low species lack a specialized nervous system.

and any other qualifications have to do with the material within the boundary of our field; they need not affect the delimitation of psychology from other sciences.

What actually distinguishes the various contemporary 'schools' of psychology is their conflicting interpretation of *partly known facts.* Why are some investigators mechanists and others teleologists? Why do some believe consciousness to be an efficient cause of action while others deny that it is a factor in the causal chain? Why do some emphasize the study of behavior and some the study of mental content? Not so much because they are working in different fields of science, as because the stimulus-response series of events is only partly known. One group of investigators bridges the gap in one way, one in another. This is natural enough, and it is entirely justifiable so long as the hypotheses adopted are recognized as assumptions. When they are used dogmatically to rule out alternative assumptions, they hamper our progress toward complete knowledge and impartial judgment of the facts. What we need at present is a demarcation of the territory to be investigated, rather than a preconception of its entire contents. We should define our field of research with reference to the known facts instead of limiting it by special assumptions. This merit is claimed for the definition here formulated.

## Relation of Mental and Physical Phenomena

If we accept this delimitation of the field of psychology, two corollaries follow. (1) Any and every scientific method of research available may be properly employed in investigating the phenomena that come within the scope of this broadly defined science. Neither introspection nor behavior-observation should be ruled out, since they deal with data apparently belonging to our field; neither should we neglect the study of the neural mechanism. (2) The assumptions of the various 'schools,' in so far as they conflict, cannot all be wholly true; some of them must ultimately be modified or rejected. What modifications are to be made, cannot be finally determined until the field is more thoroughly explored, and crucial evidence is obtained by observation and experiment.

Meanwhile it is inevitable that investigators should attempt to unify the separate groups of data by means of some general hypothesis. This is the procedure in every science. But in the case of psychology we encounter a special difficulty: What relation shall we

assume to exist between the data obtained through introspection and the remaining phenomena involved in the stimulus-response sequence of events?

Conscious experiences, mental states, seem incommensurable with the physicochemical processes which the behaviorist studies, and which form the subject-matter of natural science as a whole. This apparent disparity has proved a stumbling-block to the reception of psychology into complete fellowship with the other natural sciences. Radical behaviorism attempts to get around the difficulty by excluding 'mental' phenomena altogether from the realm of scientific investigation. But this merely shirks the issue. The task of science is to investigate every phase and all phenomena of nature. There is no question but that 'conscious experiences' do occur in the universe. How then are we to connect this apparently distinctive class of phenomena with the general system of nature—with extension, electrons, forces, organisms, etc.? In the present incomplete state of our knowledge, any theory of the relation will be largely speculative —it will rest on some 'metaphysical' hypothesis; that is, it will involve certain unverified assumptions regarding the fundamental nature of things.

Historically, at least six different theories have been propounded, based on as many assumptions concerning the relation of 'mental' to 'physical' phenomena, of which only four need be considered here.[6] Two of these theories lead to a dualistic, two to a monistic view of Nature. Parallelism and interactionism represent two opposite types of dualism. The former sees no connection whatever between the two classes of data; the latter assumes that each is able to work upon the other. Radical behaviorism is a monistic view which unifies the world by ruling out the phenomena of consciousness altogether; the identity hypothesis or double-aspect view, also monistic, reaches unity by assuming that a single underlying reality is the cause of both sets of phenomena.

Our examination of these four hypotheses[7] indicated that the double-aspect interpretation fits in most closely with the total situation as known today. It is based on the assumption that what we term 'cerebral (or neural) processes' and what we term 'conscious

[6]MTP, 271. Epiphenomenalism has been generally abandoned in favor of parallelism; mentalism or spiritualism has given place to interactionism among scientific thinkers.

[7]MTP, 271 ff.; MP, 79-85; HP, 413-41

experiences' are phenomena resulting from one and the same set of occurrences, observed by investigators in two totally different ways. The actual occurrences are certain operations of a physicochemical nature which take place in the nervous system of the organism. They may be observed more or less indirectly (objectively) by other individuals through their receptors; they may be measured and recorded by means of appropriate instruments. But these same occurrences are also events in the life of the organism within whose body they occur; the individual observes them immediately (subjectively); he lives them—experiences them.

This conception of the psychophysical relation is to be taken for just what it is worth. It rests on an assumption; it is nothing more nor less than a working tool, to be tested again and again as new data are discovered. It is not to be accepted as dogma. Neither should it be dismissed as 'absurd,' or 'inconceivable,' or 'impossible' as certain critics have done.[8] Until we get further light on the manner in which the universe operates, *any* notion or picture of the mental-physical relation is in a way 'inconceivable.' The assumptions of parallelism and interactionism are no more capable of representation than that of the double-aspect view.[9] Behaviorism alone shows an open road, and that standpoint is attained by refusing to recognize a certain group of natural phenomena, much as Christian Science ignores evil and pain.

The objections directed against the identity hypothesis are not due to any conflict with known facts, but to the difficulty of describing or picturing the process whereby the assumed single series of events gives rise to two such different phenomena as thought and neural activity. Is it just an ordinary conversion of energy? Or does it involve some sort of magical transformation? The difficulty of tracing the relation, of refuting the charge of 'magic,' appears to be the most significant objection to the double-aspect assumption.[10] But examination of the alternative assumptions shows that the same suggestion of magic appears in each—except radical behaviorism, which is blind to certain patent facts. By what occult means do two parallel but independent series come to agree and synchronize? Or how can two totally different substances interact? Whatever view

[8]Cf. William McDougall, *Body and Mind*, 1911, 155-178.

[9]MTP, 273 ff.

[10]A special phase of this problem will be discussed later; see pp. 37.

we take, we are confronted with an apparent mystery. But if the universe is a unitary affair, there is undoubtedly some non-magical way of describing the mental-physical relation. Apart from this single difficulty the identity view appears satisfactory. It seems to accord with the facts known at present. It has the merit of bringing psychology into direct relation with the entire group of natural sciences.

Furthermore it should be noticed that the double-aspect hypothesis is in line with the general trend of science. Our assumption of the fundamental identity of neural and conscious phenomena is in fact merely a special instance of the general assumption of the unity of nature, applied to two particular classes of natural phenomena.

### The Introspective Assumption and the External World

Of recent years the behavior method and behavioristic standpoint have gained many adherents, especially among the younger generation of psychologists. The claim that behavior methods alone can give exact results, has tended to cast doubt upon the value to general science of the results obtained by introspective research. This criticism would be valid if the data and principles discovered by introspection were assumed to constitute a distinct sphere of knowledge, unrelated to the other fields of science. But it is doubtful whether many introspectionists since Thomas Brown have adopted this view. Contemporary introspective psychologists proceed on the assumption that conscious experience comes to the individual through neural processes; that it is in some way a 'central nervous' phenomenon. It is worth while to examine this position more closely and determine its logical implications.

The investigator who employs introspection, treats the whole (to him) known world as his immediate, individual experience. In so far as he 'knows' the stimulus 'as such,' this knowledge is indirect; it is a conviction due to social tradition or a conclusion reached by inference. In other words, his experience includes the assumption that non-experienced stimuli exist; he believes that much of his conscious experience is due to these stimuli or forces acting upon his nervous system—though neither the operating forces nor the nervous structure and activity are directly experienced.

For introspective purposes, then, all our experiences are treated as 'subjective' phenomena; and they are assumed to be a 'correlate' or 'aspect' of definite occurrences or conditions in the 'objective' central

nervous system; they inform us about objects and relations and movements in the outer world, all of which lie outside our experience. The landscape which one sees afar off is (as a visual experience) a subjective phenomenon, even though the observer 'projects' it to a tremendous distance. This very process of *projection* (projicience) is part of the experience itself. Projection, distance, far-offness, as a characteristic of our experiences, is due to a subjective factor, like the experiential characteristic of different color hues or of different auditory tones. Perceptual space need not correspond to the geometrician's 'degree of apartness,' any more than the color scale of human experience corresponds to the scale of physical light waves. The whole field of perception lies within the observer's organism;[11] anatomically speaking it is located within his central nervous system.

What happens when the data obtained by observation are supplemented by experiment? The experimental investigator varies a certain group of experiences which represent stimuli through his motor activities, and notes the ensuing changes in other components of experience. For example, by sliding one sector of a color wheel (visual and kinesthetic experience), he alters the color experience in a certain part of his visual field. Now according to our fundamental assumption, these alterations in the color experience are actually caused by variations in the non-experienced stimuli which excited the color experience originally. The entire series of 'conscious experiences' is 'central;' it lies within the organism; but it is assumed to be generated, by the activity of external forces, or forces in outlying portions of the organism.[12]

All this means that the introspective investigator, in collecting his data by observation and experiment, is dealing with a great mass of experiences, which actually form part of his own life or being, but which he *assumes* to be due to certain stimuli or objective situations that are not themselves data of experience. Our world of experience does not include external objects, but it is moulded by external forces.

It appears, then, that introspective research, whether it proceeds by simple observation or by experiment, encounters no logical or factual difficulty when it assumes a non-experienced source of experience—an outer world—though there are obviously difficulties in representing and formulating the situation. This assumption brings

[11]Cf. HP, 416-418.

[12]Interoceptive and proprioceptive stimuli.

introspective psychology into line with biology and physics. By its means the introspective psychologist escapes from solipsism, and this without assuming an undefinable faculty of intuition.[13]

It should be noted that the so-called objective sciences tacitly make this same assumption. The physicist observes his swinging pendulum with his eyes, or studies the tracings of its swings with his eyes, or reads his tabulated results with his eyes. These various data reach his retina by physical media. Very good. But how are the retinal impressions transformed into knowledge? Does the observer's mind reach down through the optic nerve and 'intuit' the retinal occurrences, as McCosh and the Scottish realists assume? If so, what is the function of the afferent nerves? As a matter of fact scientists generally concede that the reception of data (our observation of objective phenomena) is not complete till the afferent nerves carry their 'report' at least as far as the higher brain centers. Actually, then, the objective scientist (including the behaviorist) is dealing everywhere with neural representations of non-experienced extra-organic events. That such non-experienced things exist is an assumption, but it is an assumption which the results of scientific investigation have fully justified. Granting the existence of an external world, we can study its structure and processes in the various aspects known as chemistry, physics, biology, and psychology. The objective scientist makes this assumption of externality tacitly, but the introspective psychologist must assert it explicitly, if his findings are to be brought into relation with those of other fields, notably biology.

It appears, then, that the criticism made by radical behaviorism, that the data and generalizations of introspection bear no relation to objective science, is quite unwarranted. Using the same assumption that objective scientists themselves implicitly make, the introspective observer may attribute his 'subjective' data to central nervous conditions and occurrences, which are clearly within the scope of objective science.

## The Teleological Interpretation of Consciousness

The question remains as to the precise relation existing between these subjective phenomena and the corresponding neural processes.

[13]The fact that certain experiences resist every direct effort on our part to change them, seems to have been the actual historic ground for accepting the assumption of externality and going beyond the solipsistic interpretation of experience.

According to the earlier psychologists the mind is an intelligent personality, an organized being, stationed within the body, which views the neural data and translates them into perceptions, feelings, and emotions, using this material as a basis for its own untrammeled thoughts and decisions. The obvious dependence of mental organization upon cerebral organization has led to a modification of this conception. The theory that an independent, organized mind perceives external events and directs the individual's voluntary activities has been abandoned. In its place has arisen a 'teleological' school of thinkers, who attribute much the same functions to consciousness. According to this group of investigators the fact of *awareness* has special significance; awareness enables the individual to interpret data, to choose, to act with reference to consequences.

The implications of this view have been examined at length in a previous paper.[14] Our analysis indicated that most if not all of the specific performances attributed to consciousness might be quite as readily described in mechanistic terms. The responsive activity resulting from the processes called perception, deliberation, and choice may all be accounted for without assuming a distinctive quality or potency in consciousness itself. Even conscious anticipation is seen to be the result of advance impressions received through the distance-receptors.[15] One point alone seems doubtful: "Does our realization or conviction that a certain alternative is suitable, add something to the total situation and modify the course of neural activity?"[16] On this crucial point the validity of the teleological hypothesis seems to hinge.

The evidence at hand seems insufficient to completely refute the teleological assumption; but neither is there convincing evidence in its favor. Introspectively, one experiences a 'burst of knowledge' and a feeling of satisfaction when, on comparing alternative courses of action, one realizes that a certain alternative is preferable. But there are certainly neural processes accompanying these mental phenomena, and it remains to be demonstrated that the 'realization,' apart from the neural activity, is a significant factor.

The double-aspect hypothesis, in this case as elsewhere, would *identify* the mental with the neural; that is, it assumes but one set of occurrences, manifested to different observers as 'realization' and

[14]MTP; cf. SP, part I; MP, 86-95.
[15]SP, 34-36; MTP, 276.
[16]MTP, 284.

'cerebral activity' respectively. But it must not be forgotten that the identity view, like the teleological view, rests on an assumption. Each is merely a working tool; either hypothesis may be accepted till this crucial problem is settled. This does not mean that the two interpretations are equally satisfactory. The identity hypothesis has the advantage of presenting a simpler and more unitary view of the universe. It carries out the Canon of Parsimony in avoiding a multiplicity of agencies. On this account it seems preferable to the teleological interpretation as a working hypothesis.

### The Nature of Consciousness

It would be well at this point to explain in detail the notion of 'consciousness' implied in the double-aspect or identity theory.

(*1*) According to this view neither awareness nor any of the complex forms of conscious experience, guides or modifies in any way the course of responsive activity. The form and direction of the response is fully determined by neural conditions,[17] except in so far as external forces hamper or promote its completion.

(*2*) 'Consciousness' is not a new *quale* introduced into the universe by certain complications of organization; it is merely a name for the fact that the organism is a unitary thing and is affected by its surroundings.[18] From the viewpoint of the organism, the impressions which it receives from its surroundings are its 'experiences.' In other words, consciouness means reception of impressions.

(*3*) The reception of impressions is usually not a matter of simple transmission. In man the neural activity of various simultaneous impressions combines in the higher nerve centers to form complex impressions, known introspectively as perceptions, emotions, etc. These impressions are further modified by the neural traces left by former impressions. The term 'consciousness' includes these modified data received in the central system, as well as simple impressions. Impressions received in the cortical centers constitute the 'personal consciousness' or experiences of the individual in question, and initiate responses that are known as voluntary or deliberative.

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[17]Cf. MI, passim.

[18]Even the behavioristic investigator is tied to his own body. His observation of the behavior of other organisms is confined to the locality in which his own organism is situated. His reports are dependent upon the stimuli which his own receptor organs receive. His entire research depends upon 'consciousness' as here defined.

(*4*) The activities of the lower centers do not come within the personal experience-system of the individual; they may be termed *subconscious* mental data. But there appears to be no distinction of kind between the two groups of phenomena—the conscious and the subconscious. If the introspective observer is thoroughly convinced that other human organisms besides himself are conscious, he is justified in concluding that his own lower centers are conscious also.

(*5*) The same process of reception occurs, though in lesser degrees of complexity, in subhuman species. It probably occurs also in single, simply organized cells—perhaps even in atoms and electrons. With this speculative generalization, however, psychology is not concerned. Sufficient for our purpose is the identification of 'consciousness' with the subjective phase of organic life—with the succession of the creature's experiences, due to impressions made upon him by his surroundings. Our view assumes that 'reception from beyond' occurs throughout the whole range of organic life, at least as far down as the amoeba. Consciousness is only another name for this particular kind of receptivity.[19]

Properly speaking, the term *consciousness* denotes an abstract characteristic rather than a concrete datum. The data which possess this characteristic are the individual's *conscious experiences*. It is in connection with these concrete experiences that the degree of neural organization becomes an important factor. Introspective psychology has shown that, for man at least, the number of different sensations obtainable varies directly with the number and complexity of the receptor organs; and that the number of complex experiences based on these sensations varies directly with the complexity of the central nervous system. While introspective study does not extend to subhuman species, it is generally conceded that the same relation holds throughout the organic scale. In other words, the variety of experiences open to the organism increases with the progressive evolution of receptors, effectors, and neural mechanism.

Now this multiplicity and complexity of experience involves at least two fundamental problems which must be faced by the psychologist. (1) The phenomena in the 'outer world' vary in several

[19]MTP, 268. Loeb and others distinguish between 'consciousness' a product of associative memory, and 'sentiency' or simple reception. In this discussion the more familiar term *consciousness* is applied to the entire group of phenomena.

different ways: How are these independent modes of variation transmitted to the central system, so as to produce corresponding variations in experience? (2) The phenomena of experience are characterized by a multiplicity of qualities, even within the field of a single receptor (*e.g.* the eye): How do these qualitative differences in experience arise, and what is their neural equivalent?

These are distinct problems. The former is largely a question of neural transmission, and seems open to experimental treatment. The latter is concerned with the nature of 'qualitative differences' as observed by introspection.

(1) *Correspondence between experience and the external situation.* This problem confronts us regardless of any theory of consciousness. The situation is this: The activities or forces in the universe differ in many respects. In so far as they affect the receptive organism, these differences may be reduced to three (or possibly two) classes: (a) discontinuous or non-related groups, such as light-waves, sound-waves, inert bodies; (b) serial variations of mode, such as the different wave-lengths of light; and (c) serial variations in intensity, such as the different amplitudes of light-wave vibrations.

The discontinuous groups stimulate different receptors and need not concern us here. The difficulty lies with the two other classes of difference. There are two independent variables in the stimulus: wave-length and amplitude in the visual stimuli, and some equivalent in other forces. There are two corresponding independent variables in our conscious experiences—quality and intensity. How are these two independent modes of variation communicated from receptor to center? According to recent investigations the nerve impulse varies in but one way—in temporal rate of succession; the all-or-none law seems to rule out differences of intensity or degree, except as they affect the refractory period, and that is still a matter of the temporal rate. How, then, is the second mode of variation transmitted differentially by the nerve impulse? It may be that the nerve impulse is more complex than investigation has indicated.[20] The chemical changes involved in its propagation may possibly be a more important factor than we realize, and may serve to transmit the second variable. The problem is one for neurologists to solve; and the solution is in-

[20]The second variable does not occur in muscular activity; and it should be noted that most of the investigations have been on efferent nerves.

complete unless it accounts for the transmission to the central region of the two independent modes of variation which are observed both in the stimuli and in the central pattern.[21]

(2) *Multiplicity of qualities in conscious experience.* This is quite a different problem. The experiences which the living organism undergoes, as revealed by introspection, are characterized by great qualitative variety even within the same receptive field. The series of colors, for example, consists of an array of experiences which apparently differ in sort rather than degree. The same is true of odors and of sounds. On the other hand, the constitution of the universe, as revealed by chemistry and physics, seems reducible to groupings of similar units—there is no indication of qualitative differences among the elementary data. How are we to reconcile these two findings? No satisfactory solution appears to have been offered as yet. But if the universe is a unitary system, there must be some way out of this *impasse.* Either (a) the ultimate units (electrons) are *not* actually similar; or else (b) the qualities of experience are really quantitative differences, *i.e.* differences in the number or in the grouping (configuration) of elementary neural units.

There are difficulties in the way of accepting either hypothesis. The older introspectionists (J. S. Mill and others) noted the transformation which experiences undergo when additional elements are associated, and likened this process of mental synthesis to chemical synthesis. Unfortunately for the analogy, chemical synthesis has since been reduced to quantitative terms, while the qualitative transformation of experiences remains an obstinate fact. One may venture to believe the problem solvable, like so many other perplexing problems in Nature; though the solution may still be a long way off. In any case the difficulty does not bear particularly against the double-aspect hypothesis. Behaviorism and parallelism simply shirk the issue—each in its own way. Interactionism attempts a solution by assuming a dualistic universe; but this gives rise to another problem, quite as difficult to solve: How are we to describe the process by which physical events influence the mind and mental events influence the movements of physical particles? The identity hypothesis at least faces the issue and is able to formulate the problem.

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[21] PNS, 262-267.

## Behavior and Central Nervous Activity

Attention has already been called to a serious defect in the standpoint of radical behaviorism, namely, its arbitrary limitation of the field of scientific investigation. The position taken in these papers differs from that of the behaviorists in another point, which seems open to experimental investigation. According to the behaviorists the stimulus-response process consists of a rather rapid chain of events; the motor outgo is immediate and at each instant serves to modify the group of stimuli and the ensuing response. This view seems to attach too much importance to the rôle of the motor segment of the arc in regulating and directing the form of the response. According to our view the significant factor in determining the response is the integrative activity of the central nervous system. The central nervous phenomena known introspectively as sensation, perception, thought, etc., generate the outgoing motor impulses. That is, the central nervous operations determine the form of response in the first place, though the efferent impulses are modified from time to time by kinesthetic stimuli.

As regards simple and stereotyped responses there is little disagreement; the central nervous processes are recognized as the essential factor in determining their direction. Certain motor paths present less resistance than others and the efferent impulse follows these paths. A complete description of reflex and automatized complex responses would be mainly an account of neural transmission and integration.

The crux of our disagreement lies in the nature of the neural process known to introspectionists as *thought.* May not one thought lead directly to another, so that a train of thought is capable of continuing for a period of time without being significantly modified by kinesthetic impressions? In other words, can a complex nerve 'current' pass along from center to center for quite a time without significant motor discharge and *without being significantly modified by proprioceptive stimuli?* This crucial question has been discussed in an earlier paper.[22] The problem is one of fact, and quite possibly admits of experimental solution. "If the time for accomplishing a complicated train of reasoning be so short as to preclude a succession of motor responses and return [afferent] impulses, then it would seem

[22]PNS, 257-262. The 'nerve-ring' system characteristic of radial species is of the type here suggested.

evident that the behavior phenomena are not essential to thought—that thought is essentially a central nervous operation,"[23] and not a phenomenon of motor discharge. The fact that *incidental* motor discharges occur meanwhile does not seem to settle the issue.[24] These implicit responses may or may not affect the course of the main 'current' of activity. Until this is satisfactorily determined, it is a logical error to consider the question closed. Introspection indicates that the succession of thought-experiences is extremely rapid. But this again is inconclusive evidence. The problem must be submitted to some crucial experimental test.

This leads to our final point. Introspection and behavior-observation are both legitimate methods of investigation in the broad field we have assigned to psychology. But the neurological method is after all the court of last resort. To interpret introspective data we must understand the central nervous processes. To explain the nature of the stimulus-response relation we must discover just what goes on in the nervous system. And here we face the practical difficulty that most of our investigators lack the training necessary for research in this field. Many do not understand the fundamental principles of nerve physiology. Their descriptions of nervous processes are often naïve and sometimes grotesque.[25] On the other hand, neurologists have not shown themselves particularly interested in the problems which seem most important for psychology.

Psychological investigators must train themselves more thoroughly in neurological technique, and they must endeavor to interest neurologists in the problems peculiar to psychology. The importance of biochemistry for biology is now fully recognized. Psychology has quite as much need for a group of psychoneurologists to study the fundamental processes concerned in the activities of the central nervous system.

## Summary

This paper discusses the salient features of a psychological standpoint which seems to accord with known facts and to involve the fewest assumptions.

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[23] PNS, 259-260.

[24] Is there sufficient evidence, by the way, for the conclusion that *every* stimulus issues in a response? May not a nerve impulse be blocked and transformed into heat or chemical activity?

[25] Hartley at least was abreast of the scanty biological knowledge of his time.

The field of psychology is defined so as to embrace both behavior and introspective investigations. It includes the investigation of all phenomena involved in the transfer of selected energy patterns from environment to organism and (reactively) from organism to environment by appropriate organs, and of all the intermediate processes concerned. This differentiates psychology from physiology, which investigates internal processes involving exchange of material (nutriment and waste products) between organism and environment. Our definition of the field involves no assumption regarding 'consciousness.'

The exact relation between the data obtained by the two leading methods of investigation—introspection and behavior study—is not clear in the present state of our knowledge. Various theories have been proposed, based on alternative assumptions. The assumption made and defended in these papers is that 'mental' and 'neural' phenomena are two different manifestations of a single series of actual events. This double-aspect or identity view of the world is adopted as a suitable working hypothesis, not as a demonstrated theory. Consciousness is regarded as neural receptivity; conscious and subconscious experiences are 'central' data, whether simple or organized, which result from stimulation.

The points of disagreement between this view and others are examined. The processes claimed by teleologists to demonstrate the efficacy of consciousness are reducible to mechanistic terms, except for one doubtful instance: "Awareness that a certain alternative is suitable" may possibly alter the direction of responsive activity. The evidence in this case is inconclusive, but the double-aspect view attributes the effect to the neural conditions involved in awareness. It thus avoids assuming a multiplicity of agencies. This consideration of simplicity gives our identity assumption the preference over any form of interactionism. Parallelism is objectionable in that it fails to account for the synchronism of two distinct systems of events.

The chief difficulty in the identity theory is to reconcile the qualitative variety observed among conscious experiences with the multiplicity of similar units to which objective science has apparently reduced the universe. But this problem confronts all other theories as well, except the behavioristic. Another difficulty is to account for the neural transmission of two independent modes of variation (wave-

length and amplitude) which are observed both in the stimuli and in the central (conscious) data. This latter appears to be essentially a neurological problem.

Our criticism of radical behaviorism is two-fold. It ignores or rejects certain phenomena—conscious experiences—which require scientific investigation, and it assumes that the character of the response is determined by the motor discharge of neural energy, rather than by the integrative central activity. The latter criticism involves a question of fact, which appears open to experimental solution.

Finally, while introspection and behavior-study are valid methods of research in the field of psychology, it is pointed out that both are really subordinate to the neurological method. Our chief need at present is the systematic development of a science of psychoneurology, by means of which we may accurately describe the fundamental operations concerned in reception, integration, and response. Biology has summoned chemistry to its aid. Shall we not call upon neurology for similar assistance?

# WHAT IS NATIVE IN THE SO-CALLED INSTINCTS?

By W. B. Pillsbury, University of Michigan

The doctrine of instinct is in a state of flux in psychology and sociology at present, if not indeed in a state of chaos. Even the behaviorists who might be expected to be its best friends have put it on the door step of the biologists. The sociologists, who would naturally be jealous of a fixed line of action that could not be influenced by the social forces, deny that it was ever born. The *Gestalt* group mix it so thoroughly with the other orphans of a voluntary and intellectual type in the baskets of the *crèche* of configurations that it might as well be disowned completely. In brief the child that has descended from such a noble line and was once awaited with such hopeful anticipation is now disowned and discarded by almost all, by all of the really aspiring and *soi-disant* respectable schools. Nevertheless, it is a healthy, even a well-favored infant and if possible a home should be found for it.

The old notion of an instinct was that it consisted in a chain reaction, which differed from a reflex only in being more complicated or longer drawn out—a series of responses bound together by the fact that each movement started the next one in the series, rather than a single response. It was assumed that the entire series was innate and was performed as soon after birth as occasion arose, as perfectly the first time as after practice. Recently the entire process has been denied validity, and most agree that such definitely fixed chains of reaction are very rare in man and even in the higher animals. Watson's results may be taken as showing that in young infants only very few and very simple acts are entirely innate. Nevertheless there is a certain harmony or unity in the response of different individuals of a species to the same stimulus that must find some explanation.

If we accept the results of Watson and others as proving that relatively few of the even moderately complicated responses of the higher animals and especially of man are fixed at birth, we must choose between three current theories as an explanation of the relative uniformity in the responses of the adult members of a species. We have the suggestion of the behaviorists that all is learned and uniformity comes wholly from imitation and social pressure. This would eliminate all but a few simple responses that might be called

reflexes almost as well as instincts from a possible list of innately determined activities. A second hypothesis that would reconcile an assumption that much is innate with great progress in accuracy through learning is Allport's suggestion that we have certain latent tendencies that may be developed to full-fledged habits more readily than others. These he calls prepotent habits since they are developed more readily than other acts that are not prepared for. A third suggestion is that tendencies to acts may be innate, but show themselves only after some years when the nervous system has developed sufficiently. This is the theory of delayed instincts that has had many exponents at various times.

Each of the theories assumes that much if not most of each instinctive act develops after birth or is learned. They differ only in the explanation that they give for the unlearned parts. Neurologically, movements are dependent upon the synapses that chance to be open at birth between an afferent neurone that happens to be stimulated and the efferent neurones. If but one is open or one is much more open than the others, we have a reflex. If several are approximately equal in ease of access, one movement will follow another in the tentative trial-and-error sort of way, so familiar in all learning. Both the reflex and the series of tentative movements are innate. Granted that several movements are possible, still the one that involves the path offering the least resistance would be favored at birth and would be taken first. Later others will open and the corresponding movements will be made. Where several movements are involved in the execution of a complete act, the completion of one movement would join with the stimuli present to constitute the stimulus for the next. This chain of responses is the simplest instinct in the traditionally accepted picture as we may find it realized in the animal.

In man, as in the animal, the basis of all that is later acquired must be found in these neurological connections; but the variability of response and the fact that some one of the possible connections becomes realized in the act that is finally established as habit make it certain that this is not the whole story. There must be some way of selecting between the successive movements aroused by the same stimulus or situation. That one result should be accepted and the others rejected needs an explanation. This itself cannot be due to learning; rather it is the basis of learning, the condition that antedates learning. Even if one accepts all as conditioning of reflexes,

that is, as due to the transfer of something from one stimulus to another, there must be something that made the effects of the original response acceptable, permitted it to be repeated, and which could be transferred as response and accompaniment to the new situation. The fact that certain foods are retained and start the swallowing movements, while others are at once spewed out, is fundamental. Something similar makes the starting point of the explanation of all selection from among the possible movements. We ordinarily say that these movements that are chosen satisfy or are pleasant, while the others are unpleasant or do not satisfy. This may mean no more than that they tend to be selected and established. That alone is observed. However it is expressed, certainly movement after movement will be made until one comes that produces a particular effect. That will then be repeated at first in a majority of cases when the stimulus is presented and then each time the stimulus is presented. When it is repeated each time the stimulus comes, it is said to be learned.

Even if we accept the view that aside from a few fundamental reflexes all man's movements are learned, we find two innate factors that determine the learning. First is the fact that connections are easiest between each of the stimuli and one movement, and secondly the fact that certain movements, when they appear, will not be repeated while others tend to be repeated. It is as if the organism were trying to attain a goal and would accept only those chance responses that attained or tended to attain that goal. The organism is, of course, not aware of the goal, need not even be aware that it is making movements that go towards the goal. One can only say that certain responses are repeated until they come to be established. This process of learning that is universal in the animal kingdom up to and including man we know as learning by trial and error.

Not merely are all acts made possible by innate connections between nerve elements, but groups of movements may be favored in learning by the inherited connections. Thus it is possible that such a combination of movements as walking may be furthered by the nervous connections. This might be expected to show itself by the readiness with which the series of acts becomes established when the suitable stimulus is given at the proper age of the child. This statement involves two parts: that groups of neurones might be connected so as to produce a chain of movements, and that this connection may not show itself on the first stimulation, but only later. If

it showed itself later there would be two possible explanations again. It might be constituted by a series of connections so slight that they could only be developed by practice, or these connections might not be present at birth but come with the ripening of the nervous system, that is as a result of changes that developed after birth as the nerve tissue grew. The first of these theories has been suggested by Allport under the name of prepotent habits, that is, of habits that may be formed more easily than the average. The second is the theory of delayed instincts.

If we take the case of learning to walk, Allport's theory would be that there was no tendency to make the walking movement when the group of stimuli was applied, even when the muscles were strong enough to support the body and the nervous connections were fully developed. Still, when by chance the combination of movements that constitutes walking happens to be made, it would be established by fewer repetitions than would a habit that was not favored by the connections in the nervous system. It would be learned in the same way as any other movement but more readily. Different from this is the theory of delayed instincts only in so far as the connections in the nervous system are supposed to be established without trial and without practice. On this assumption the pressure of the ground upon the foot coupled with the sight of a bit of candy a little in front will at two years of age at once arouse the group of walking movements without trial. The fibers in the neurones involved are assumed to have become capable of carrying a nerve impulse, or the synapses between sensory and motor neurones have become open at this age where before they were closed.

Actual observations indicate that one view will fit the facts about as well as the other. Kirkpatrick and Woodworth have both reported children who had been kept from walking until past the ordinary age for the development of the movements and then walked without preliminary trials on the first occasion that they were permitted to make the attempt. These cases favor the delayed instinct theory. Breed and Shepard tested the pecking response of chicks. They selected two lots of newly hatched chicks. One they tested at once. They counted the number of times each chick hit the mark as it pecked at a bit of cornmeal, and the total number of pecks. The ratio of hits to tries measured the adequacy of the movement. The second lot of chicks was fed by introducing food into the bills and kept in the dark even during the feedings for from one to five days

and then were tested on successive days for the adequacy of the instinct. The results showed that the chicks kept in the dark until the connections might be regarded as ripened made as many mistakes on the first day of trial as did the newly hatched, but progressed much more rapidly until by the eighth day all were on the same level irrespective of the amount of practice. Since the movements were not adequate at the first attempt, one might assert that the explanation by prepotent habit was more closely supported than the delayed instinct that made it possible for the stimulus to evoke the response more accurately in the older chicks, *i.e.* a tendency for the impulse to pass more frequently over the suitable path than over the alternative paths. If the instinct develops with growth, it is not a fully fledged instinct but merely an ability to learn more quickly.

At the other side of the problem, what is innate may be nothing more than the restlessness, the tendency to make series of movements as long as conditions are unsuitable. This might be regarded as the drive or as one phase of the drive. The stimulus would be hunger, or the accumulation of hormones from one or more of the ductless glands, or some chance stimuli might act upon the body mechanism well supplied with nourishment—what has sometimes been called the working off of surplus energy. That is merely another statement for the fact that all animals tend to move if there is not excessive fatigue. Stimuli constantly coming in from the sense organs furnish the occasion for each movement, but they are disconnected and the movements show no indication of being directed to any end.

The third factor in the instinct is the selection of some one of the chance movements or the results gained from a series of movements. This selection again may mean nothing more than a quiescence when the source of restlessness is removed, or it may be the beginning of a new and appropriate series of movements. The chick that has satisfied its hunger turns to something else. If one think of the pecking alone as the reflex result occasioned by the hunger stimulus, then swallowing takes place, provided the substance taken into the bill is suitable to be swallowed; otherwise it is rejected. The selection between the food stuff and others is again on the basis of innate capacity and so might be called instinctive. If we assume that an originally acceptable object becomes unacceptable because it is associated with an unsatisfactory or repulsive stimulus, at least the first stimulus must be regarded as instinctively or originally unacceptable.

If then we analyze the so-called instinct into its final components, there prove to be three of these: the incentive to action, the movement that results, and the test of acceptability which induces repetition or continuance of the movement, or determines that it shall not be repeated or continued. Now each of these separate parts may be in part at least regarded as inherited tendencies. The restlessness is certainly a function of the fundamental protoplasm, and may possibly differ from race to race and from individual to individual. The movement that results is fundamentally due to connections between neurones, and again varies from race to race and from species to species. Both the first movement and the ease with which movements may be established are innately determined. The test of acceptability also must be in large measure immediate, and what is learned can be no more than a transfer of what is a native reaction to one stimulus to another like it or that produces the same response. The child immediately accepts certain substances as food and rejects others when put into his mouth. He may transfer the readiness to accept from the food taken into his mouth to the sight of the food, and seek that which looks like that which gave a pleasant taste. On the other hand food which in itself is pleasant or at worst indifferent may have been connected with an unpleasant sensation and be disliked for a long time. A bit of shell in the first oyster may make the child reject all oysters for years. The innate element is the objection to the sound or impression of the shell between the teeth.

Each of our three components of instinct or of a learned movement has a fundamental innate element or factor. The first is largely specific as to presence but can result in a large number of different responses. The nature of the response is indifferent to it. It compels to do something, but anything satisfies it. The second, the movement, is fairly closely limited as to the responses that shall be made by the sensori-motor connections. Theoretically one response must come first and the others follow in a definite order. In case of a strong stimulus, one movement will be made in greatest intensity or extent, and others will be grouped with it in ever decreasing amplitude as they spread from the center of discharge to related muscle groups. Not only the first movement to be made, but also the ease with which connections may be fully established so that finally one carefully adjusted movement alone will result, may be regarded as determined at birth. Most completely innate is the satisfaction that

the movement induces. This may be changed by conditioning, but is present in each individual and is fundamental to all others.

How adequately the three factors combine to give a definite movement varies. In some instances as in swallowing and nursing, the chain of movements results at once. We have what might be called a pure instinct. In others, as in the pecking by chicks, the movements are rough and random at first but soon become fully established and standardized. In others, as in games of skill, the first movements are little like the adequate ones and must be gradually developed from the crude first attempts to give a smooth performance. The difference between an instinctive movement and a learned one is in degree alone. Each has certain elements that are determined at birth and each requires learning by trial and error. No sharp line can be drawn between the learned movement and the instinct. The extremes are quite different and probably deserve separate names, although giving the names should not be acccepted as indication of a complete distinction.

In all questions of the inherited factors, call it instinct or what we may, we must distinguish between the initial restlessness and the final satisfaction, on the one hand, and the movement that attains the result, on the other. We can, I think, say definitely that the inherited element is to be found in the restlessness and the factors that afford satisfaction rather than in the movement itself. Barring reflexes, where all is determined by the nervous connections and the movement results accurately and mechanically when the stimulus is applied, every movement is learned by the tentative process called trial and error. Some are learned quickly, some only after many attempts. We may recognize in this some sign of prepotent habits. We may also see some evidence of delayed development of nervous connections that passes under the name of delayed instinct. In the latter, however, we must also recognize that what is delayed in appearing may be the physiological basis of the restlessness or the goal, rather than the movement itself. In pecking, and in lesser degree in walking, the evidence still leaves room for actual development in the connections that make possible the movements.

If we may largely eliminate the actual movements from the factors that are inherited, the question arises as to whether we can ascribe the main element in the drive to the initial restlessness or to the satisfaction given by the result of the movement. Obviously both must work together. There would be no movement unless there were

some incentive and the satisfaction that is to be attained at the end of the movement cannot be regarded as foreshadowed in advance of its attainment. It serves only to stop the series of random movements. The types of occasion for restlessness may be the relatively mild ones we have mentioned above, hunger, hormones from the sex and other glands, or they may be intense excitations of the sense organs, such as loud sounds, great pain, or the sight of large and rapidly moving objects. According to Kretschmer and others before him these are responded to at once and, he asserts, reflexly or instinctively either by vigorous badly coördinated movements or by rigidity and quiescence. The former are exaggerated trial and error movements. While the connections formed in these excited conditions are not so conducive to learning as the ones made in response to milder restlessness, some learning probably results. It might be asserted with some degree of plausibility that reactions to man in the mass follow one or the other of these types. Certainly the paralysis of first appearance conspicuously before a crowd or even an audience might come under that head. Flight results less often. We are assuming that these excitors do not drive to any particular movements, but that all arouse indefinite general responses. These might be regarded as the real drives. In themselves, however, they lack direction.

Equally important are the stimuli that put an end to the responses. In eating, the final cessation of restlessness would come with satiety. Before that the acceptance of the taste of food as it is taken into the mouth would be an important factor. If satisfactory the swallowing movements succeed those of locomotion, and the eating is continued until the food is gone. If not satisfied restlessness recurs and movements begin anew. The restlessness or the quiescence on the part of the child before a group disappears after a time. If it runs, the running ceases when out of sight. If it is paralyzed, the quiescence disappears when it is no longer observed, or after a time, when nothing dangerous happens. Again we have the immediate pleasure of social approval that serves to make restlessness of all kinds disappear, just as social disapproval may cause renewed indefinite and exaggerated movements. These stimuli act as important determiners of learning. The fact that many of them act when first applied in the life of the organism would indicate that their effects also are innate. Again there may be transfer through learning. What would serve to induce quiescence or acceptance of a situation at one time, may later become

connected with after-effects that produce restlessness and so change the first effect upon the organism. For the first effects they must be looked upon as inherited determiners of action.

So far we have been treating the whole problem with reference to the responses and their physiological determiners alone. It is essentially the behavioristic approach. After the restlessness has led to action a few times and a specific act has been produced, the original stimulus tends to become connected with the final results. We can see this or at least state it most simply if we turn from the behavioristic language that we have been using up to this point and speak in ordinary consciousness terms. The restlessness at once arouses a memory of the satisfying character of an older solution. This leads to the suitable movement without the preliminary trial and error of the inexperienced animal. What McDougall and Woodworth call the 'drive' is a compound of the original stimulus that would produce restlessness plus the final solution that is constituted by the allaying of the restlessness. For the conscious experience the two are fused to form a single element that we are accustomed to call pleasure or satisfaction. We usually neglect the preliminary discomfort if the outcome is satisfaction and speak of the total drive as the striving for or the attainment of an end.

In the animal we may assume that something of the same sort also takes place that may be given a behavioristic statement in terms of the conditioned reflex. The stimulus that produces restlessness becomes connected with the movement that finally produces the stimulation that in turn causes quiescence after a longer or shorter series of connected movements. We may speak of the original stimulus becoming conditioned by the outcome to induce the movements that lead to the final result of quiescence or the series of other quieter, straight-ahead movements that succeed the violent diffuse ones. In animal learning of the maze the stimulus for the original running about becomes connected with the eating movements that lead finally to satiety. The terminal stimulus becomes connected with the initial stimulus; in behavioristic terms it serves to condition one direction of running rather than another. The two become fused and together constitute the drive to action.

One would like to know why one stimulus results in the terminal quiescence or series of quiet movements. We are using these terms as a non-committal substitute for Thorndike's satisfaction. That it is in the long run a native rather than an acquired effect seems fairly

clear. We may trace back each component to a factor conditioned by something else, but that something else seems finally to be native. The nastiness of the taste of the cinnabar caterpillar for Lloyd Morgan's chicks conditioned the sight and finally prevented pecking at them. That the nastiness itself with the induced response of rejection rather than swallowing is fundamental and innate to the chicks is hard to question. One here approaches the old evolutionary theory of feeling as an explanation of how they come to be inherited or to have developed by survival in the race. We need not mention feeling if that offend the behavioristically inclined. All that is essential is the fact that certain taste stimuli induce swallowing movements, and that others induce the rejecting or expelling movements. Feeling might or might not be present and, if present, might be cause or effect, so far as our argument at present is concerned.

We only need to insist that there is a native connection between certain stimuli of widely different sorts and groups of responses. Some of the responses are simple and definite, others are more general, and in certain cases may merely facilitate or inhibit the action of other stimuli. In the former list come the qualities of tastes, less directly the sight of foods, in the latter the approval or disapproval of the group. The approval of the group leads to quiescence, the disapproval to paralysis or continuation of vague struggles. A complete list would include most of the elements that are commonly treated under pleasure and pain. That we need not attempt to give and could not if we would.

We see there are three phases of an act that may be native. In my opinion, much of the disagreement in the modern discussions of instinct comes because different authors keep their eyes fixed upon one or two alone of three necessary components of an instinctive act. Most have emphasized the connection between the stimulus and the response. It is true, as Bernard and Watson insist, that in relatively few cases is this connection complete so that the movement is made perfectly the first time. Most of these instances would be classed as reflexes rather than as instincts. Equally important in determining action are the effects of external and internal stimuli to initiate vague movements, the restlessness from which learning proceeds. This, too, is innate although not distinctive and so of little value for learning. More important is the effect induced by stimuli that are effective after the chance movements have been made and which serve to initiate new movements or to terminate the restlessness. These also

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have effects that are immediate and may be regarded as depending upon the innate nature of the organism. In that sense they are truly instinctive elements.

If we examine any list of instincts with a view to see which of these three parts has most importance, it is seen that Watson's grasping, defense reactions, nursing, all belong in the group of connections between stimulus and response. Manipulation would have more of the preliminary restlessness in it. Rage and fear of falling among the emotions would also fall into the second class. Love as the response to contact on an erogenous zone would be the only one in which the effect could be assigned to the third group of inducing a cessation of movement. Most of McDougall's instincts would have the innate factor in this inherited effect of a terminal stimulus. Of course he gives no analysis and makes no recognition of the three phases, but the parental instinct, curiosity, sympathy, acquisitiveness, the herd instinct, the constructive instinct would all have their determining factor in the response to the completed act rather than in the immediately evoked response. As treated, self-assertion and submission might well be the opposed effects of Kretschmer's paralysis and the diffuse response. Mating would involve both the preliminary restlessness and the final satisfaction. This can be seen very clearly in the analysis that is given by Thorndike and Woodworth. With this difference of emphasis which amounts to a difference in definition of what shall be called instinct, the talking at cross purposes of recent writers becomes readily explicable.

If we ask whether instinct is still a useful term, the answer will depend upon the presupposition. We may assume that there are so few instincts, if defined as immediate unlearned responses, that we can eliminate that definition. What there are may as well be called reflexes. On the other hand, the number of activities that are determined by a preliminary restlessness or discomfort, which continues until a stimulus is chanced upon that initiates quiescence or a definite series of responses, is so great that the class covers almost all activities. The only question is whether the group responses that result from these factors are sufficiently clear cut to be worthy of separate names. If we retain the term we must admit a considerable indefiniteness of acts for each instinct. We must also grant that in the course of learning many of the separate satisfactions become connected or conditioned one by another, so that if we assume a theoretical unity or simplicity of stimuli that check the restlessness, these

elements become much complicated in practice. We must also grant that habit or learning in general has a large part in uniting the preliminary restlessness with the terminal satisfaction. The only innate characteristics are the effects of the original condition to induce restlessness, and the effect of the terminal stimulus. Both the movements that make the organism pass from one to the other, and the association that makes it anticipate the second when the first is given are acquired.

After all this is granted the term instinct is convenient as indicating that something of the behavior of the individual is determined at birth. The names that are given to instincts will be largely names of pleasant or unpleasant terminal stimuli, what we call ends. In fact most instincts are at present named either from the ends or from a state that leads or drives to action rather than from the acts. The term 'end' has been avoided in the discussion and should be avoided in general for it seems to imply that the terminal condition draws or determines the act. This it does only by the conditioning in and through experience. There is nothing in it but the tendency of some terminal stimulus to destroy the restlessness either directly by producing quiescence or by starting a series of movements that removes its cause.

# ENVIRONMENT AND CONTEXT

By Madison Bentley, University of Illinois

The correlative terms 'organism' and 'environment' suggest one of the great and fundamental categories of modern biology. They imply that life involves two intimately disposed systems, the living body and "the resultant of all external conditions which act upon it." This conception of an intimate and active association in which the organism is focal and the environment a theater of objects and forces contributing to, but apart from, life has had a profound effect upon the history of the biological sciences as well as upon psychology. Many problems of structure, of physiological operation, of development and growth, of heredity and evolution, and of mind and intelligence, have involved this dynamic relation between what is within and what is without the limiting surfaces of the living system.

The historian of the biological disciplines will remind us that the current conception of an active environment, to which the living being perpetually adjusts itself, came relatively late; that it became explicit and important only toward the close of a long era during which plants and animals were independently observed in comparative isolation from their surroundings. Problems of morphology, type, species, internal function, and even geographical distribution were generally approached with the observer's eye directed toward the organism itself rather than to the active and constant dependence of the creature upon outside energies and objects.[1] But the historian may also remind us that the evolutionary conception of the environment as an active agent goes back at least as far as Lamarck, that Lamarck accorded an important place to it in his *Philosophie zoologique* (published just a half-century before the *Origin of species*). Changes of environment encourage—as Lamarck thought—the modifications of use and therefore those habits which result in alteration of form and of organ. For him plants were *directly* modified by environment, for they had "no activities and consequently no habits;" but animals *indirectly*, through habitual uses and disuses.

[1]Cf. J. V. Carus, *Geschichte der Zoologie bis auf Joh. Müller und Charl. Darwin*, 1872, 594, 724; H. F. Osborn, *From the Greeks to Darwin, an outline of the development of the evolution idea*, 1894, 128 ff.

Thus is to be explained by the influence of diverse environments the wide variety of irregular forms which stand outside the straight line of descent from the simplest and most imperfect to the most complex and perfect creatures.[2] Our historian might recall further that the elder Geoffrey St.-Hilaire rejected the Lamarckian principle of use and habit for his own contention (following a suggestion of Buffon) that the environment (*le monde ambiant, extérieur, le milieu ambiant*) has an *immediate* effect on the structure of animals; and it is, therefore, a direct agent of evolution. With him, then, the external world is to be set down as a factor coördinate with the organic 'germ' in determining life and form.[3] With Darwin's principle of competition from other organisms, as well as from more direct sources, the environment entered as the primary agent in evolution; and at about the same time Herbert Spencer conceived life as the correspondence of organic changes with changes without the organism.[4] Physiology studies—as Spencer maintained—the physical correspondence and psychology studies the psychical. Physiology thus deals with one set of active and dynamical relations between the organism and the environment and psychology is a coördinate study of the same kind. Since Darwin and Spencer, at least, the conception of the environment as an energetic and effective agent, essential to the functions and the history of the living organism, has been dominant both in biology and in psychology.

To be sure, the activities of animal life long ago called attention to the external requirements in the form of matter and energy. Cuvier, regarded life as a constant flux, in and out, by which the organism maintained its enduring form in spite of a ceaseless change of materials. But this outside source is scarcely the 'environment' of our Darwinian and post-Darwinian biology, which implies, so to say, a novel and active creature of great complexity and high integration which is as various, as dynamic and as adjustable, in its way, as the organism itself. This environment is less a reservoir in which life floats and feeds than an active complement to the body and a condition *sine qua non* of life. In our current thinking this environment presents to the individual organism highly selected and stable configurations; the stability involving many chemical components

[2] *Philosophie zoologique* (H. Elliot, Eng. trans.), 1894, 199.

[3] *Mém. l'Acad. Roy. des sci. de l'Inst. de France*, 1833, xii, 63–93 (esp. 68, 69, 79–81).

[4] H. Spencer, *The Principles of Psychology*, 1855, 491, 506.

delicately proportioned among themselves, as well as suitable physical conditions of temperature, light, pressure and the like. From the particularity and selectiveness of these outside configurations, which play differentially upon individual organisms and kinds, it follows that the current environment is less a common outside 'nature' (*le monde ambiant*) than a specific organization of objects, events and energies diversely conformable to the necessities and conveniences of organisms and suitable to specific interchanges and influences. So it comes about that unlike organisms may have the same habitat but different environments; *e.g.* the traditional scholar at his books, his bustling servant, the drowsy dog under his chair and the parasitic afflictions of all three are quite differentially environed. Henderson has made out an impressive case for the general fitness of the environment to life, and we might go further in maintaining an elastic and adjustable fitness of substance and energy for providing at once many suitable environments for many forms and fashions of life. Were our contention allowed, we might affirm as corollary thereto that that increasing complexity of organisms which has come to our knowledge through high specialization in the sciences of life is wellnigh paralleled by a new revelation of environmental complexities. Such a brilliant volume as the recent Yale studies of adaptation[5] cannot fail to impress us with the vast intricacies of the world when the world is set as environment over against the living being. There the tendency is to reduce the 'scenery' of the world to the raw factors of moisture, temperature, light, chemical action and mechanical agents, and then to observe the infinite interaction and configuration of these agents in the actual ecological setting.[6] The effect of these environmental factors upon metabolism, growth, development, behavior and racial descent has long been, and doubtless will continue to be, variously estimated and interpreted; but the basal fact of organic dependence upon outside agencies is firmly established Our own interest here lies rather in the limits of usefulness and propriety of the concept of environment than in any attempt to deny its relevance in morphologic production, growth, behavior, and hereditary change.[7]

As regards the limitations of the concept, it may first be observed that the designation of an environment often confuses outside con-

[5]M. R. Thorpe and others, *Organic adaptation to environment*, 1924.
[6]*Ibid.*, 4, 17.
[7]The eternal debate upon the primacy of heredity *versus* the primacy of environment, the sciences of life may well leave to the lay dogmatists.

ditions which are effective for the organism with mere neighboring objects and events, *i.e.* with *le monde ambiant.* If our terms imply a real and dynamical relationship, then it is foolish to regard the watch in my pocket as a part of the environment of the submerged starfish which I observe from the shore. Much of the surroundings of the living organism is not really environment, however the observer's perception may bring it into a specious relation with the creature under scrutiny. Effectiveness is itself ambiguous. Thus the green tree and the white wall are effective objects to me but not to the earthworm in my path. To the earthworm gradations of light are effective, not the shapes and patterns of my solid and significant objects. This is a confusion common among those students of animal activities who take for granted that *their* environment is identical with the environment of the animal which they observe. For objects, energies and changes to be really environmental to an organism they must be effective agents regarded in just the way which they actually *are* effective. It is quite foolish to speak of the hornet 'responding' by a sting to the fear of the timid and excited passerby when it is only responding to certain temporal changes in light-pattern and possibly to olfactory stimuli. Words are a part of *my* environment but not of the environment of the fish which I hope to lure by my bait. Vibratory movements of the water may well be environmental for the fish but certainly not words. If we are to take seriously the selective, dynamic and effective agency which is environment, then we must regard it only in so far as it is actually accessible to the organism in question and only in the precise and factual way in which it engages the living creature. One of the commonest logical errors of the observer who describes the activities of the animal in terms of stimulus and response is to substitute an object of his own perception for the physical agent which is actually operative upon the organism under observation. Thus a sophisticated apprehension of the experimenter by himself may be substituted for the actual objects which appeal to, and which are effective for, the young infant undergoing examination; and the socialized observer of the white rat may wrongly put himself down as the object apprehended by, and casting an influence upon, the lowly animal.

In the second place, the separation of the organism and environment at boundary lines and surfaces is, in certain cases, arbitrary and conventional. The symbiotic relationship offers an example, and so does the parasite which is lodged within the host and is not therefore

really external. Neither is the nutrient material ingested into the body cavities really environmental. It would be difficult to define the exact moment when food-stuffs become part of the organism and cease to be 'foreign' materials; and on the other hand, the exact passage from organism to environment of rejected glandular and digestive products and of residues expired from the lungs is equally indeterminate. Again, in the active interchanges of the developing germ-cells and the rest of the body do we have the same essential relations of a contextual field but with both terms within the same organism; and a like relation may be observed for those materials which are transported in the *milieu intérieur* of blood and lymph streams. Once more, in our body-coverings, hand-tools and weapons do we have 'outside' attachments which might well—save for our arbitrary delimitation at the rind—be functionally partitioned with the organism, quite as much as hair, claws and teeth, instead of with the environment.

We do well to realize that the vogue of the environment was strongest before the functional complexities of the organism were clearly apprehended. At the recent dedication of the Whitman Laboratories, Professor Jennings, the champion of internal arrangement, recalled the partial failure of Loeb and other environmentalists to describe the behavior of organisms as the resultant of outside forces acting upon tropistic and other simple and invariable mechanisms of the body. Jennings contended that a knowledge of surrounding media and energies—however detailed and accurate—was wholly inadequate. "Biological experiments," he said, "that limit themselves to analysing the environment and cataloguing the immediate effect on organisms of its components, will not lead far into biological science. Only when combined with adequate consideration of the other fundamentals does it become an instrument for such insight."[8] One of these "other fundamentals" is the physical and chemical study of the substances of the organism and another is the study of the gross and minute arrangement or organization of the bodily structures. All serious students of life know how much experimental support Jennings has himself brought to his contention that the same organic materials "under the action of the same agents, respond in most diverse ways depending on how the materials are arranged." Therefore "the responses of organisms need not at all correspond in

[8]H. S. Jennings, Biology and experimentation, *Science*, 64, 1926, 101.

their diversity to the diversities of the agents which act upon them."[9] It is to the neglect of internal arrangement and organization, with a corresponding exaggeration of environmental factors as the determiners of organic activities, that Jennings ascribes the "grotesque failure" of many promising experiments in biology.

A like neglect and exaggeration are to be found among those psychologists who experiment and write in terms of stimulus and response. Here the environment occupies the stage, the organism coming in only as an imperfectly known link between incoming and outgoing energies, which are wrongly equated as environmental cause and effect, stimulus and response. I hold with those (and here the adherents of the doctrine of the *Gestalt* may be included with many others) who look upon the organism as yielding functional totalities under the conjoint action of internal and external integrations and not as merely transmuting outside 'stimuli' into outside 'responses.' Under this view the atomistic conception of the environmental relation is rejected for integrative resultants which shift and change with the whole physical configuration of inside and outside factors.

But over and beyond these various imperfections and deficiencies of the environmental point of view, we must note a more significant and general limitation of our correlative terms.

Environment, as we commonly use the term, is only a particular case of a far more inclusive juxtaposition. Whether the student of life is concerned with the whole organism, the functions of a single organic system (*e.g.* vascular or nervous), a single organ or tissue, the individual cell, a nucleus, a chromosome or a congeries of genes, he has always to take into account the *context* in which his material inheres. No part, segment or element of the living body exists and functionates by itself, any more than the total organism maintains itself in isolation from the circumambient objects and energies which we look upon as the environment. Whenever, then, we consider living substance or process in its natural setting we must of necessity deal with context. It appears—at least to an interested observer from a neighboring field—that a very large part of the current biological studies, whether in genetics, cytology, morphology, the physiology of digestion and gland, osmotic transfer, the embryonic development of axes and gradients, of germ and soma, or in the study of neural transmission and cerebral fields, implies the interaction of focus and context, of context and focus.

[9] *Idem*, 101-102.

A similar prominence of these correlatives is to be seen also in psychology. All those studies upon focus and margin, upon figure and ground, upon interacting fields of perception, upon anticipation and outcome, upon organic cores in emotion, and upon the progressive stages of action, understanding and thought, imply that experiential facts, too, are to be comprehended not in isolation but in terms of coherent connection between the item abstracted for observation and the context in which it is set. Also where the psychologist has to take account of the bodily factors of experience, internal contexts again everywhere appear. Such contexts include glandular influence upon neural and digestive functions, the dependence of nervous metabolism upon circulation, the advance and spread of nervous impulses, the coöperative and inhibitory action of spinal and cerebral structures, common boundaries of cortical fields of excitation, and the synergies of remote areas of the brain.

To establish in more general terms the concern of the psychologist with the animal organism (whether human or non-human) we shall do well to cite for our guidance three heuristic principles. The first of these is the principle of *adequate surface interchange* which defines the psychological status of a given form in terms of those arrangements (receptors, effectors, manipulative and exploratory devices) of the body which promote and facilitate the apprehension of environmental objects, agents and events. Notable instances are the exploring hand, the binocular visual mechanisms and the devices for swift movements of bodily advance and retreat. The second principle is *plasticity with fixation.* Under that principle those forms which are most susceptible to variable functions with permanent residues therefrom (as by way of memory) stand—other things equal—higher in the range and significance of experience than less plastic forms. Here the fruits of habituation and the residues of learning appear. Finally, the third principle, the principle of *unity and integration,* leads us to look for psychological advancement among those organisms which are best provided with structural and functional means of integration and government. Here we follow the path of neural centralization and cephalization, of expanding synaptic systems and superior cortical control.

Now it is apparent that only one of these three principles—that of adequate interchange—bears *directly* upon the environmental context, while the other two suggest internal and intra-organic mechan-

isms and functions.[10] To regard only the first; to consider only the outside relations and adjustments; to deal, let us say, only with the operations of stimulus and response, is to deal inadequately and superficially with the complexities and the intricacies of the living and experiencing organism.

In both coördinate branches of the sciences of life, the biological sciences and the psychological sciences, the plain fact is, then, that men have learned to study the organism organically. The terms of this organic observation may be as limited as cell-body and nucleus or as broad as total body and environment. In any case, the organic relation is fittingly implied in our general term 'context.' The grosser context of the environment will continue to suggest its problems; for it is at the outer boundary that we observe the setting of the greatest organic unit that we know. But more and more, as it appears, will the integrative study of the *internal* relations and unities continue to reveal the most intimate facts about the nature of living beings. We certainly gain in logical clearness and simplicity when we realize that all of our studies of inner and outer relationships fall under the single category of context.

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[10]Among recent writers few have escaped the limitations of the environmental context so successfully as has Professor Washburn. In this author's *The Animal Mind* the focus of observation falls almost invariably within the organism itself rather than upon extra-organic objects and agents. And again in the brilliant essay upon *Movement and Mental Imagery* this writer has attacked with high success certain of the most difficult problems of functional organization. Both of these works notably advance our knowledge of the issues of organic plasticity and developmental integration.

# *GESTALT* PSYCHOLOGY AND THE CONCEPT OF REACTION STAGES

By R. S. Woodworth, Columbia University

My real object in the present paper is, I am afraid, to glorify the stimulus-response psychology, or that variety of it which appeals to me and which has room for 'mental reactions.' The view that all processes within the organism are responses to stimuli may appear so platitudinous as to be pointless. I should like to show that this view has teeth. I would have it try its teeth on the *Gestalt* psychology—not, indeed, on the concrete experiments and working program of the *Gestalt* psychologists, which are certainly well worth while, but upon their system, with its generalizations and exclusions.

Let me start by outlining the view that appeals to me. When an external stimulus impinges on the organism and elicits a motor response, there is undoubtedly a series of intraorganic processes intervening between stimulus and movement, sometimes a relatively short series, as in reflex action, and sometimes a long series, as in the skilful handling of a complex situation indicated by the stimulus. These intervening processes do not form a simple chain, but they can be schematically represented as a series of stages. Now the relation of each stage in the total process to that which just precedes it is the relation of response to stimulus. Each stage is aroused by the preceding stage and in turn arouses the stage which follows. The later stage is not a continuation, or development, or organization, or re-adjustment of the preceding stage, but a fresh activity involving, no doubt, fresh neural or muscular mechanisms. Some of the stages of the total reaction may be called 'mental,' which does not imply in the least that they are transcendental and incapable of description in terms of neural and muscular activity, or even, conceivably, in terms of the motions of electrons and protons, but simply that it is more profitable, for the present, at least, to describe them in such terms as "recognition of an object," "thought of another object," "desire for a certain result," etc. These mental events are responses to what precedes them in the total reactive process, and they are also the exciting causes of what follow them. They are reactions. They take time; undoubtedly each has a latency or reaction-time, for each is the ac-

tivity of a different mechanism. Each adds something new to the total reaction. Each stage has characteristics of its own, and is different from the preceding stages, just as muscular contraction is different from the activity of sense organs.

The object of this conception of reaction stages is not in the least to minimize the enormous neural complexity of the whole process, but to point out that the total reaction is not a continuous flow of energy through the organism, but the awakening to activity successively of different parts of the organism, each part being awakened to its own type of activity, though all these activities have this in common, that they are responses to stimuli reaching them from other parts of the organism, or from the environment.

A typical, fairly direct total reaction includes the following stages, following in order upon the external stimulus: sensation, perception, intention, muscular contraction, movement of limbs and of external objects manipulated. The last stage is not a reaction in the strict sense, since it involves no new activity of living cells, but it is an essential stage in the whole process, and deserves mention on the same plane as the external stimulus.

Now I wish to set up four men of straw, in order, by contrast, to make the features of the reaction stage theory more clean-cut and attractive. I fully admit them to be men of straw, though I believe the caricatures will be easily recognized. These four are the sensationist, the perceptionist, the intentionist, and the motorist. Each of these psychologists is so fascinated with a single reaction stage that he seeks to interpret the whole reaction in terms of that one stage.

For the sensationist, perception and intention are compoundings of sensation, while muscular contraction lies outside of the field of psychology. The stage of sensation provides all the elements, and the later stages consist of different aggregations and emphases of these elements. The questions to be asked regarding any percept or intention concern the elementary sensations of which it is composed, and the relative clearness or prominence of each in the compound. The sensationist can point with legitimate pride to many excellent studies of the senses and of the sensory data on which various perceptions are based.

The perceptionist denies the sensationist's multiplicity of elements, and looks with disfavor on the whole program of analytic study. Surveying the field from the base line of perception, he sees everywhere things, events, wholes with form and meaning, not to be re-

solved into constituent parts. In place of analysis, he proposes a program of investigation into the conditions favorable or unfavorable to the recognition of patterned wholes. Intention he regards as the drift of perceived patterns towards greater symmetry or more stable equilibrium. Even motor learning is essentially this same perceptual process.

The intentionist sees everywhere conation, desire, wish-fulfillment. The play of motives is omnipresent in life, and nothing else seems worthy of consideration. There is nothing mechanical in behavior, but always wishful doing. The only thing worth studying is what the individual wants, and how his different wants interfere with each other. The organism is a bundle, or system, of desires, which infallibly reach their goals, except for the interference of other desires. The organism has, in fact, no machinery to be considered, but only driving forces.

The motorist is so impressed with the importance and objectivity of the motor response that he denies the importance, or even the existence, of sensation, perception and intention. If there are stages in the process leading from external stimulus to muscular contraction, they are simply stages in the motor response. The organism has no characteristics save those of movement (and of glandular activity), and will be fully understood when the characteristics of motor response have been sufficiently examined.

Now each of these men of straw represents a fruitful line of psychological investigation, but for that very reason each *system* of psychology is obviously false. It is absurd that psychologists generally should be expected to choose between such one-sided and fanatical systems. What is needed is a system that shall have a logical place for every sort of scientific psychological work, and the framework of such a comprehensive system seems to be provided by the concept of reaction stages.

The concept of stages of reaction is certainly not new. It is probably the view of most psychologists, though not all would look with any special favor on the particular list of stages given above. It is almost certainly the view of physiologists, who speak of a muscle fiber as excited by its motor nerve fiber, of the dendrites of one neurone as being excited by the axon terminals of another neurone, etc. Each structure has its own type of activity, and at any moment is in possession of a certain amount of immediately disposable energy which, according to the all-or-none law, it consumes completely in

the single response. The physiological concepts of stimulus threshold, of summation of stimuli, of latent time, and of refractory period, are all bound up with the general notion of response to stimulus.

The most active opponents of the stimulus-response psychology at present are the *Gestalt* psychologists, caricatured above as the 'perceptionists.' This group, in their general theory, seem to ignore the all-or-none law, intraorganic thresholds, synapse resistance, and everything that bespeaks that relative discontinuity which is implied in the notion of stimulus and response. They wish to think of the physiological mechanism as a complete continuum, similar to a network of charged wires. This system, being at a given moment at equilibrium within itself and with the environment, is disturbed by an intercurrent force, whereupon a shifting of internal tensions occurs till a new equilibrium is reached. Possibly this conception can be harmonized with accepted physiological laws, though it certainly appears inconsistent with them.

The *Gestalt* conception seems specially inadequate to deal with the effects of a plurality of stimuli. Consider first the case of two simultaneous stimuli. Let two bright lights suddenly appear in the field of view, one to the right and one to the left of the present fixation point. If either light were alone, 'equilibrium' would be reëstablished by a turning of the eyes towards that light. Since both lights act at once, the principle of a continuum of forces would require us to conclude that the two effects would balance each other according to the parallelogram of forces, so that the eyes would remain staring straight forward, or come to rest at some intermediate position, according to the relative intensity of the two lights. Nothing of the sort occurs, however, but the eyes fixate one or the other of the two lights, and later, very likely, shift to the other. This principle of alternative responses, or of reciprocal inhibition, is illustrated also in spinal reflexes, in binocular rivalry, in the staircase and other ambiguous figures, in selective attention and selective motor response, and is absolutely characteristic of the behavior of the organism. Plurality of simultaneous stimuli is universal, and selective response is equally universal. The physiological *Gestalt* theory simply does not take care of this line of facts.

Succession of stimuli is almost equally universal. Before equilibrium could be reëstablished after the first stimulus, it would be disturbed again by the second, and then immediately by the third, so that no equilibrium could be reached, and behavior would have to

be an uncertain teetering to and fro, with no definite reactions of any sort. What really happens is an overlapping in time of the responses to successive stimuli. The first stimulus starts a response process on its way through a series of stages; the second stimulus, supervening before the motor response to the first is reached, starts another response process on its way; and so on. In repetitive work, the successive responses, being much alike, succeed each other much like a series of waves; and overlapping them simply means that at any time there is more than one wave progressing through the organism from sense organ to muscle. In other cases, the response to the first (or any) stimulus may be delayed in its completion, by passing through several central stages before it reaches the muscles; and then the response wave to the second stimulus reaches the centers while they are still responding to the first stimulus; and thus the two responses influence each other. Complications of this sort may occur to any extent without disturbing the general scheme of reaction stages.

Overlapping has been best demonstrated in linguistic processes, such as receiving telegraphic messages,[1] typing from copy,[2] and reading.[3] First a bit of material is vaguely sensed, then more clearly, it is 'organized' in the light of the context, the motor response is planned and finally executed in detail. There may be a span of six, eight, or ten words between the receiving eyes or ears and the executing hands or voice. Now the incoming telegraphic clicks do not simply fall into a cistern, to emerge as finger movements some moments later. Something is being done to them at every instant during this interval. They are fed into a machine which performs a series of operations and turns out a finished product at the other end—only there is no material passing through the machine, but only a series of responses.

Now a cross-section of the organism's activity at any instant would cut across several total reactions, one at its earliest stage, another a little further advanced, another nearly completed. One might be sectioned at the stage of sensation, the one before it at the stage of perception, the one before that at the stage of intention. If we then described the organism's activity as seen in cross-section, disregarding the fact that we had cut across several response waves,

[1] W. L. Bryan and N. Harter, Studies on the telegraphic language: The acquisition of a hierarchy of habits, *Psychol. Rev.*, 6, 1899, 346-375.

[2] W. F. Book, The psychology of skill with special reference to its acquisition in typewriting, *Univ. of Montana Publ.*, 1908, 1-188.

[3] F. M. Hamilton, The perceptible factors in reading, *Arch. Psychol.*, 17, 1907, no. 9.

we could say that the organism's activity showed the multiplicity of elements characteristic of sensation, the configuration characteristic of perception, and the forward-striving characteristic of intention; and it would depend on the bias of the individual observer which of these characteristics he stressed. Something very much of this kind has happened in introspective observation and led to the divergent assertions of the different schools. The sensationist can say that, no matter what state (cross-section) we examine, we shall always find sensory elements present. The configurationist retorts that, no matter what state we examine, we always find configuration, and never elements devoid of pattern.

The *Gestalt* psychologists have probably proved their case, to the extent of demonstrating that perception (as well as intention and thinking) involves configuration, and that the configuration is not to be described as the aggregation of elements. But they are not content. They wish to show that there is no unfigured activity of the organism. Therefore they deny the reality of sensation, as an unfigured stage in the total respose.

The doctrine of sensation, which the *Gestalt* psychology rejects, and which I should accept, runs about as follows. The sense organs are composed of receptor units, which are pretty well insulated one from another, so that the activity of each is for the most part unaffected by the activity of the others. Further, the nerve paths leading to the sensory centers from the various receptor units are insulated from one another, so that the receptors are projected point for point upon the receiving sensory centers, and what these centers receive consists of discrete stimuli from the various receptor units. So far, there is multiplicity, but no unity, no figure and ground. Configuration begins from this point on, and amounts to a reaction of other structures to the convergent multiplicity of stimuli provided by the sensory apparatus.

The configurationists could accept this doctrine, and still retain all the psychological configuration there is. But they wish to rule out the unfigured stage altogether. So, going outside the organism, they point to the existence of real, dynamic, physical configurations, such as the net-work of electrified wires aleady mentioned. Another good example would be a ball rolling on the ground, since the motion of every particle in the ball is determined by the motion of the whole ball. Finding configuration to exist outside the organism, they sug-

gest that it passes by some continuous flux into the organism, so that there need be no unfigured stage in the organism's response.

Now it can be shown that physical *Gestalt* is neither necessary nor sufficient to insure perceived *Gestalt*. The 'phi-phenomenon' is an instance of perceived *Gestalt* without any physical *Gestalt*. The apparent movement in the motion-pictures is another example, since there is no physical motion on the screen. The constellations which we so readily see in the sky are not astronomical systems, but simply aggregations of luminous points that happen to lie in nearly the same direction from the earth. On the other hand, the solar system, which is a physical *Gestalt*, is not readily perceived as such, but the conception of its dynamic unity has to be built up by piecing together discrete facts. The rolling ball we do not readily see as a unitary process, but we do not see, and entirely overlook, the friction of the ball on the ground, which is essential to the physical *Gestalt*, as without it the ball would not roll. It would seem, then, that the presence of physical *Gestalten* in the environment is irrelevant to the perception of *Gestalt*, and that we are still free to believe in an unfigured sensory stage in the total response of the organism.

What is meant by calling the sensory stage unfigured can be understood from an analogy. Suppose the central weather bureau desires to get a picture of the barometric condition of the country at a given time. It calls upon its observers, at scattered stations, to report the barometric pressure at their respective stations. These several reports, assembled in the central office, make possible the construction of a pressure map, which shows the whole state of pressure. This map is a *Gestalt*, clearly enough. But the observers who supplied the data for the map were isolated one from the other. In fact, it was essential to the true picture that each barometer, and each report to headquarters, should be independent of the rest. Now the receptor units are the organism's sensitive instruments, and it is essential that each respond truly to the stimulus affecting it alone.

From the time of James we have heard that sensory analysis is an artificial type of experiment, and that it does not really analyze the complex percept that it sets out to analyze, but simply demonstrates that one can, with practice, get another experience from the same stimulus. This is about what the *Gestalt* psychologists say, in objecting to the sensationist's type of work. Yet one cannot dismiss such achievements as Helmholtz's analysis of a clang into fundamental and overtones as mere laboratory 'stunts.' What Helmholtz

did was to sort out the elements in the unfigured sensory stage of the total response. He did not analyze the clang, if by clang we mean the total quality of a complex tone. In general, sensory analysis is not an analysis of percepts, intentions, or other figured stages of response, but an analysis of the sensory stage of response. It is important, nevertheless, in the study of perception, because it reveals the data on which the percept is based, or, we may say, the immediate stimuli to which the percept is a response.

# THE PROBLEM OF ORIGINALITY IN SCIENCE

By Edwin G. Boring, Harvard University

Of all scientists the psychologist ought to be the one who is best prepared to take a knowing account of the personal equation as it enters into psychology and thus to understand the history of his science, for the history of science is primarily a history of thought, and the modes of human thought are something that a psychologist ought to understand. Yet psychologists, both in polemical argument and in historical evaluation, are apt to be quite uncritical of personal and mental factors that are essential to an understanding of the situation.

This unpsychological nature of psychological criticism shows clearly in the perpetual attempt to assign credit for discoveries or for new and original theories and systematic conceptions. Was Fechner or Wundt or someone else the 'father' or 'founder' of experimental psychology? Or was Fechner the 'father' and Wundt the 'founder'? Did Wundt found the first psychological laboratory at Leipzig in 1879 or was the cigar-box of tuning-forks under Stumpf's arm an earlier laboratory? Did Stanley Hall found the first laboratory in America at Johns Hopkins in 1883 or was James' room at Harvard in the seventies with the frog-rotator, the charts, and a few other things the first? It is with this matter of *originality* that I wish to deal, and I have chosen my examples from the history of physiological psychology.[1]

Now for all that has been said about creative imagination I take it that psychologists are pretty well agreed that thought is seldom strikingly discontinuous and that novelty in thought arises normally in a deliberate process of development and, even when it seems to be abrupt (as in wit), is largely a function of the past psychological history of the individual. If this picture is true of the individual, it must also be true of the genealogical group which contains him. Scientific thought is the thoughts of scientists, where one man takes both from others and from his own past, selects, and perhaps by novel combina-

[1]The entire history of science should properly be my subject-matter; but I am writing for psychologists, and the history of physiological psychology is a present interest with me.

tion creates. "The stream of consciousness is sensibly continuous" in the history of thought as well as in the thinking of an individual. Certainly the historian is impressed by the fact that almost never does an idea seem entirely new. If it is a great idea that has helped to make a name or a date great, he looks for its previous occurrences. Generally he finds them. Not always can he be sure that the early instances actually fathered the great emergence, but often, when he is not sure of the fact of inheritance, he is also not sure of its absence. The bases of one's own present thought are often indeterminate; how much more difficult to form a judgment of the thought of another ten or a hundred years ago! Thus the historian may come to suspect every case of apparent originality. If he realizes, however, the tedious process by which thought develops, and by which novelty, after flitting about for years in indirect vision, is finally fixed for a brief space in the fovea of science (new thoughts do not occur; they hardly emerge even; they evolve), then he becomes aware of the grounds for his suspicion.

With this view of scientific evolution, what becomes of the 'fathers' and 'founders' in science? It is my intention to remind the reader by a few psychologists' instances, that the 'fathers' are necessarily also 'sons' and that the 'founders' are very apt to be 'promoters.'

(*1*) *The specific energies of nerves.* Right or wrong this doctrine has played a very important rôle in the history of physiological psychology. Helmholtz is said to have attached to it the importance within physiology that is given to the law of gravitation within physics. It lies at the bottom of Helmholtz' theories of vision and hearing and of Hering's theory of vision; it set the problem and method for the discovery of sensory spots on the skin in the eighties; it is the only generally accepted theory of sensory quality that we have ever had; and in general it supports and implies the projection theory of the nervous system and the theory of brain centers that have been orthodox gospel at the end of the last century and the beginning of the present. There is no doubt about its importance. How much of this credit should we give to Johannes Müller?

Sir Charles Bell is the other posthumous claimant for the origination of this theory. In 1811 Bell privately printed "for the observation of his friends" one hundred copies of a little pamphlet of less than six thousand words, entitled "Idea of a New Anatomy of the

Brain."[2] It was circulated privately and was little known until reprinted in 1869 (and again in 1911). It contains the original statement of Bell's law (or Magendie's, for this is another controversy for priority) of the sensory and motor functions of the spinal nerve roots; but it also contains a discussion of sensory nerves which anticipates almost every statement that is essential to Müller's doctrine of the specific energy of nerves.

Müller published first in 1826 in connection with his studies on the physiology of vision,[3] but the view hardly gained the status of a 'doctrine' until his elaborate exposition and the formulation of the ten laws in the *Handbuch* in 1838.[4] Here Müller's exposition occupies more than six times as much space as Bell's.

In mere point of time Bell was first. However, the theory has always been referred to Müller and, in contradistinction to its extension by Helmholtz, it has sometimes been called the Müllerian doctrine. Müller does not refer to Bell in this connection, although he knew Bell's work. Whose theory is it?

It is plain, I think, that the answer cannot be categorical. Bell was first in time, but he may not have been the psychological ancestor. Müller is so honest in giving priority to Marshall Hall in the case of the reflex, in spite of his own independent work, that I do not believe that he knowingly got the doctrine from Bell.[5] In fact, it is only when one knows Müller that one can easily see the full significance of Bell's remarks. What Müller certainly did was to formulate the theory, to 'promote' it, to give it a complete statement and a systematic setting, to insist upon it, and to lend it the authority of his own immediate personal influence and the equally great influence of his *Handbuch*. He is certainly responsible for the 'doctrine' as a doctrine, and to call it the Müllerian theory is not so greatly to falsify history. Bell's account of these same facts was too casual, too obscure, seemingly of too little importance to its author, for Bell to insist upon the importance of the view as did Müller.

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[2]Charles Bell, *Idea of a New Anatomy of the Brain submitted for the observations of his friends*, 1811; reprinted in *J. Anat. and Physiol.*, 3, 1869, 154-157; and in original English with German translation as *Idee einer neuen Hirnanatomie*, 1911.

[3]Johannes Müller, *Zur vergleichenden Physiologie des Gesichtsinnes*, 1826, 44-55; *Ueber die phantastischen Gesichtserscheinungen*, 1826, 6-9.

[4]Müller, *Handbuch der Physiologie*, II, bk. v, introduction (1838).

[5]L. Carmichael, *Psychol. Rev.*, 33, 1926, 198-203, champions Bell as against Müller, and suggests that Müller may have known Bell's remarks on the subject.

Nevertheless there still remains the question as to whether the view was psychologically original in Müller's thought, even though it had been less positively stated earlier. This is a difficult question to answer from the documents; still I think some light will be thrown upon it if we shift the ground and enquire: Was the theory original with Bell?

To the modern reader, who does not have to combat the common sense of physiology of the early nineteenth century, Müller's ten laws and exposition seem verbose and repetitious. The laws seem to reduce to seven fundamental principles, and the case for Bell's priority can be founded on the fact that for Müller's extended treatment of each principle a concise statement to the same effect (sometimes only a sentence) can be found in Bell.[6] Let us consider these principles separately.

(*a*) The first and fundamental principle is that we are directly aware, not of objects, but of our nerves themselves; that is to say, the nerves are intermediates between perceived objects and the mind and thus impose their own characteristics upon the mind. There is, however, nothing new in the idea that the nerves are intermediaries between the external world and the brain; that had been said by Herophilus and Erasistratus (ca. 250 B. C.) and had been common doctrine since Galen (ca. 200 A. D.). That the nerves thus impose their own nature on the mind is a necessary consequence of the materialistic view of immediate causes and of the brain as the organ of the mind. Epistemologically this principle in Müller has been characterized as "a fruit of the anthropocentric standpoint, as the newer philosophy from Descartes down to Kant and Fichte has developed it," and also as the mere "physiological counterpart of a Kantian category." If we would transfer Müller's priority to Bell, it is irrelevant to remark that Bell was probably but little influenced by Kant, though Kant died in 1804. We do not have to go to the Continent. This principle is plain in Locke's doctrine of the secondary qualities (1690) and in the psychophysiology of Hartley (1749). It is essentially the physiological mechanism for English Empiricism. Bell may not have been interested in British philosophy but it would be strange if he could remain innocent of its influence

[6]Limits of space prevent the detailed comparison of Bell with Müller as well as the more complete exposition of Bell's antecedents (*infra*). Both must wait upon the publication of my projected history of experimental psychology.

two centuries after Locke. Philosophy becomes common sense slowly, but, when it persists within philosophy itself, inevitably.

(*b*) The second principle is that there are at least five kinds of nerves and that each imposes its specific energy or quality upon the mind. Is this principle after all so very much more than Aristotle's doctrine of the five sense-qualities, added to the modern view of the nerves? Vision has for its sense-quality color, hearing has sound, and so on; such is Aristotle's doctrine. It would seem almost that Bell and Müller had returned to Aristotle and to the obvious fact that one sees with the eyes, hears with the ears, and feels with the skin; and that the return was difficult only because of the well-intrenched but less obvious doctrine of the nerves as indifferent, passive conductors.

(*c*) The third point of the doctrine asserts that the same stimulus affecting different nerves gives rise to different qualities appropriate to the particular nerves, and conversely that different stimuli affecting the same nerve give rise only to the peculiar quality for that nerve. It is hardly more than the formulation of the evidence for the first two principles. Müller himself believed in the antiquity of this notion and cited Plato, Aristotle, and Spinoza in support. Both Bell and Müller drew instances from common sense (*e.g.* the colors seen on pressing the eye-ball) and from recent physiological experiment. Volta, the inventor in 1800 of the Voltaic pile and thus in a sense of the electric current, had described experiments showing how electricity could arouse the different sense-qualities. The evidence was meager and scattered, but it was there. Both Bell and Müller merely brought it together.

(*d*) Müller insisted, fourthly, upon the equivalence of external and internal stimuli. This is a point that has definite significance only as long as it is not taken for granted that the brain is the immediate organ of the mind. As soon as this latter view is admitted, it becomes obvious that internal stimuli, like external, are mediate to the mind and thus in the same relation to it as external stimuli. Bell took the point for granted and did not deal with it specifically. Both men write at a time when common sense was changing and the notion of the peculiarly mental function of the brain might or might not be considered obvious. But neither of them of course originated the view. It is very old; it was fundamental in Hartley, for instance; and early in the nineteenth century it was being established in common sense by Gall and Spurzheim and phrenology. That Müller,

more than a decade after Flourens had assigned different mental functions to different parts of the brain, should feel it so necessary to insist upon the basic relationship is a little surprising.

(*e*) Müller found it necessary to explain in his laws how external objects can be perceived correctly, in spite of the fact that the mind is immediately aware only of the state of the nerves. Bell too gave a similar explanation. The discussion runs along the lines of what we now-a-days call adequate and inadequate stimulation. The general problem, however, is as old as the problem in idealistic philosophy, and was not original as problem or solution with either.

(*f*) Then both men raised the question of the exact locus of the specificity of the nerves. Bell assigned it definitely to the brain. Müller agreed that it must be either in the termination of the nerve in the brain or at least in its central part. Both appealed to the evidence from cut nerves. I do not know that this specific point had been definitely anticipated. It follows naturally, however, from the preceding argument, and it is implied in the numerous suggestions of physiological philosophers, from Descartes on, who have looked to different parts of the brain for different mental functions; in other words, it is of a piece with the theory of sensory centers in the brain.

(*g*) Finally, for the sake of completeness, we must note that Müller in his last law dealt with the selective power of the mind upon the perception of sensory impressions. Even in Bell we can find a single sentence to parallel the tenth law of Müller. The point hardly belongs among the laws of specific nerve energies, but at any rate it was not new, but as old as the problem of attention among the philosopher-psychologists.

What I have tried to show is that Bell's originality lies not in the enunciation of any new principles, but in the selection and collocation of certain old ones which gather force when brought thus into relation. How much of this Bell though out explicitly and how much he absorbed unconsciously from philosophy-become-common-sense we shall never know, nor does it much matter. I have no desire to detract from his originality. My thesis is rather that originality is just this sort of affair, a matter of selection, of collocating, of emphasizing relationships, and of clarifying exposition. Such, I take it, is the psychology of originality in the individual and thus in science at large.

Müller then was less original than Bell only in that this same body of thought was more firmly established fifteen and twenty-five years

later when he wrote, and thus more likely to transcend the limen of consciousness in Müller. It is just possible that Bell also had contributed to the body of thought from which Müller more or less unwittingly drew. To Bell's contribution, as we have seen, Müller added explicitness, documentation, systematization, authority, and publicity, that is to say, he made the view into a 'doctrine' which could have a name and quite naturally therefore came to be named for Müller.

Yet whose doctrine is it? Bell thought of it first, but Müller thought of it later and got it introduced into the body of science. I do not care how the reader decides. I have gone thus into my first example at length because it is the type of all. I believe that almost every new idea in science has been as little new as the doctrine of the specific energy of nerves in Bell—or Müller.

(*2*) *Helmholtz' extension of the doctrine of specific energies of nerves.* In the extension of the doctrine of the specific energy of nerves we find a situation similar to that in the origination of the theory. The original 'Müllerian' theory dealt with the differentiation of the five senses. Its amplification to apply to qualitative differences within the single sense-department is usually spoken of as "Helmholtz' extension." Helmholtz, as is well known, used the amplified theory as the basis for his theories of vision (1860)[7] and of hearing (1863).[8] In vision, since all the colors can be obtained physically from three, he assumed, following Thomas Young, that there must be three specific visual energies. There is no such simple physical rule for the tones; in hearing therefore he assumed that every just noticable tonal difference must have its specific energy, several thousand in all. It is probably this amplification and use of Müller's principle that has done most to fix the general doctrine in physiological psychology.

This extension of the theory was fairly obvious. Was it original with Helmholtz? No, for it had been specifically proposed by Natanson and A. W. Volkmann in 1844,[9] when Müller's exposition in the *Handbuch* was still only six years old. Natanson had laid down the general principle that for every organ of the nervous system there

[7]H. L. F. v. Helmholtz, *Handbuch der physiologischen Optik*, 1866 (this section, 1860), 192-208, 290-293, 839f.; reprinted in *idem*, II, 1911, 3-20, 119-122, and Eng. trans., II, 1924, 1-21, 142-146. Cf. also *idem*, 1896, 349.

[8]Helmholtz, *Die Lehre von den Tonempfindungen*, 1863, Abth. i, end of Abs. vi, or Eng. trans., Pt. i, chap. vi.

[9]Natanson, *Arch. f. physiol. Heilkunde*, 3, 1844, 515-535; A. W. Volkmann, Wagner's *Handwörterbuch der Physiologie*, II, 1844, 521-526.

can be but a single function and that there must therefore be as many organs as functions, that is to say in the case of sensation, as many kinds of nerves as sensory qualities. This principle has in it the possibilities of greatness; it was not destined, however, to play such a rôle. Natanson applied his theory to touch, taste, smell, and color. In the case of color he assumed, like Helmholtz, the necessary three specific energies. He lacked Helmholtz' courage in assuming several thousand energies for hearing; since there is no known way in which all frequencies can be gotten from a few, he left the problem of hearing open. In every other respect he and Volkmann anticipated Helmholtz, and Volkmann at least wrote in a source with which Helmholtz must have been familiar.

The extension of the original theory, I have said, was obvious, although neither Bell nor Müller made it. It was so obvious that it occurred to Thomas Young in 1807 before there was any theory to extend. Young suggested that "each sensitive point of the retina" must contain "a limited number of particles" that vibrate in resonance with the frequencies of "the three principal colours, red, yellow, and blue." It is possible, he thought, that "each sensitive filament of the nerve may consist of three portions, one for each principal colour."[10] Now no one would claim that Thomas Young proposed the amplified theory of the specific energy of nerves in his explanation of color vision, and yet it is plain that Natanson's principle and Helmholtz' theoretical practice of finding a nervous structure for every elemental quality was a natural and obvious way of thinking to Thomas Young in 1807.

It appears then that, in so far as the historical process of origination goes, Natanson and Volkmann bear the same relation to Helmholtz in the extension of the doctrine of the specific energy of nerves that Bell bore to Müller in the doctrine itself, and that Thomas Young plays the part of some of the anticipators of Bell. The principal differences are incidental: the extension of the theory is not considered a great event; neither Natanson nor even Volkmann was, like Bell, a great man and neither, like him, had posthumous supporters who rise in defense; Helmholtz showed no desire to consider his amplification of the theory important or to claim originality for

[10]Thomas Young, *Course of Lectures on Natural Philosophy and the Mechanical Arts*, 1807; for the theory of color vision, I, 440; but the quotation concerning the three portions of the nerve filaments is an obscure isolated sentence, remote from the color theory, II, 617.

it. In both cases, however, the development of thought was gradual and not abrupt, and the fact that there has been no argument in this second case merely raises the question as to whether there should be in the first.

(3) *The Bell-Magendie law.* The controversy between Bell and Magendie concerning priority in the discovery of the law of the spinal nerve roots[11] is famous.[12] Bell's original report of the facts is in the obscure monograph of 1811 already cited.[13] Magendie published in 1822.[14] The situation was somewhat complicated at the time by the fact that there was also a third claimant, Alexander Walker, who published even before Bell in 1809, but we know least of him and historians seem content to regard him simply as a plagiarist, whether conscious or unconscious one does not know. At any rate Bell announced his discovery in his lectures in 1807 and Walker had attended the lectures. Moreover, Walker got the facts reversed from what Bell and Magendie found and later work has confirmed! In general the discovery has been regarded as an independent discovery by both of the principal claimants: the finding is called the Bell-Magendie law. Those who are sticklers for priority call it Bell's law. Bell could not, of course, have gotten any hint from Magendie. One of Bell's students, Shaw, put the discovery in 1821 in a book which Magendie eventually read. Could he have been influenced by it or by earlier knowledge? No doubt the argument from priority for originality gains its force because it is never possible to be quite certain that a rediscovery is quite as original as the first discovery; there may at least have been obscure psychological factors operating.

For myself I am content to leave Magendie co-equal with Bell in this matter. He was certainly not a cheap plagiarist, though none of us can always be sure of all his sources. What seems to me most important is the fact that the dates we are considering mark the beginning of a remarkably fertile period of research in the physiology of the nervous system in both England and France. It is difficult to say what started growth except that the time was ripe; science in general and physiology in particular had come to the stage where

[11]That the sensory nerves arise in the posterior, and the motor nerves in the anterior, roots of the spinal cord.

[12]It has, moreover, recently been quite satisfactorily discussed by Carmichael, *op. cit.*, 191-198.

[13]Bell, *op. cit.*

[14]François Magendie, *J. de physiol. expér. et pathol.*, 2, 1822, 276-279, 366-371. He discusses Bell's priority and his own independence on pp. 369 ff.

research could be effective. Some say that it was this discovery of Bell's that began the whole process; but it seems to me that, if Bell had not made the discovery, someone else, if not Magendie, would. The distinction between movement and sensation was obvious, even if the nerves were thought to be the common conductors of both kinds of impulses. The cord and its nerve roots were more accessible in operative technique than the brain. It was a great discovery to find that there were two kinds of nerves respectively in the two kinds of spinal roots, but nevertheless it came out of the times and belonged to the times. If it had not been made in England, it would have to have been made in France at about that time, and Magendie first, and Flourens second, were the logical persons to make it. It seems to me most likely that the question of originality in Magendie is simply the question as to whether he had heard indirectly about Bell's belief and, perhaps forgetting the detail, had had his mind especially directed to the experiment. On such slender threads does the formal question of originality depend.

(4) *Localization of function in the brain.* The history of scientific opinion as to the localization of function in the brain is a history of an intimately continuous development and one that is especially instructive because it seems at present to be returning upon itself. It begins of course with the establishment of the brain as the principal organ of the mind. We have noted above that the Alexandrians (Herophilus and Erasistratus, ca. 250 B. C.) and Galen (ca. 200 A. D.) held this view. Most people suppose that Descartes held it, although, as a matter of fact, he held specifically that the soul finds its seat in the entire body as a whole and that the pineal gland is simply the point at which it and the body interact. Nevertheless, on the Cartesian view, the brain becomes the primary organ of the mind for it is here that the soul, directing the course of the animal spirits, determines movement and is in turn affected by perception. The Cartesian brain is potentially a mass of mental 'centers.'

With the empiricism of the eighteenth century there grew up the view of the brain as the seat of mind with impressions conveyed to it and motion conveyed from it by the nerves. Hartley (1749) is most explicit, and one gets with him the impression of different associable vibratiuncles (and therefore ideas) as localized in different parts of the brain. It seems to have been phrenology in the early nineteenth century that fixed upon common sense the notion that the brain is the sole organ of the mind. The physiologists were in advance of

common sense, though in general opposed to phrenology. Bell certainly took the relationship between the brain and the mind for granted, but Bichat (1800), while placing the seat of most mental functions in the brain, had assigned the emotions to the internal organs. Phrenology, for all the opposition it aroused among the scientists, helped to fix scientific opinion on this point.

However, no sooner was the matter of the seat of the mind practically settled, than investigators immediately began to raise the question of the specific seats of its particular functions. Rolando (1809) sought, on experimental evidence, to attribute particular functions to the cerebrum, the cerebellum, and the medulla. Flourens' experiments (1824-25) on this problem are classic, and one tends to date the experimental work on localization from him, though once again one finds an incomplete anticipation of an important discovery; this time it is Rolando's priority over Flourens. The latter, using the method of extirpation, assigned separate functions to the cerebrum, the cerebellum, the corpora quadrigemina, the medulla, and the cord, insisting, however, at the same time upon the unity of the entire nervous system and the persistent interrelation of the parts.[15] Every one of these parts had, it is true, its *action propre*, but it has also an *action commune*. Within each part, however, Flourens found no specific localization; extirpation of a part of the cerebrum, for example, did not cause the loss of a general function nor the loss of the ability to reacquire a particular function. In fact Flourens' general view of the cerebrum is surprisingly like the 'equipotentiality' of Lashley and the *Gestalt* of Köhler to-day.

In spite of Flourens thought tended in the middle of the nineteenth century toward a more specific localization. Phrenology may have made some impression while being disproved. The doctrine of the specific energies of nerves favored localization, because it seemed that the specificity resided in the central termination. The improvement of the microscope and the discovery that the brain was a network of fibers connecting cells—an anatomical picture that resembled the associationistic psychology—favored it. The French psychopathologists made the view seem more plausible by their examination of the brain in a search for the cause of mental disease. There were many incidental facts or theories that told for localization. Finally in 1862 Broca discovered the speech center at the base of the third

[15] M. J. P. Flourens, *Recherches expérimentales sur les propriétés et les fonctions du système nerveux*, 1824; *Expériences sur le système nerveux*, 1825.

frontal convolution of the left cerebral hemisphere.[16] This event can be taken as the first discovery and proof of exact localization, but it is noteworthy that it is simply an incident in a continuous development.

In 1870 Fritsch and Hitzig mapped by the electrical method the motor centers[17] and there followed immediately a period of great activity in England, France, and Germany, in which the modern theory of cerebral motor and sensory centers was developed. Quite recently, however, the work of Franz and then of Lashley[18] has led to great doubt about the existence of precise centers and thus to a hesitating return to a position that in its broad generality resembles Flourens'.[19]

Now who discovered exact localization of function in the brain? Descartes? Some empiricist, like Hartley? No, we are accustomed to say, they had no direct evidence; they were guessing. Bell? He was not sufficiently explicit. Gall? He was explicit enough, but wrong. Flourens? Perhaps, but he was not sufficiently specific. The physiologists who thought in this manner between 1840 and 1860? They were not explicit enough or emphatic enough, or else they lacked the evidence. Broca then? Yes, by a lucky chance, a readiness to see the significance of his good fortune, and a method which others were using without his luck. His discovery of course is wrong, but I take it that a discovery does not have to be true to be great. Though wrong it led to further discovery. Fritsch and Hitzig? Yes also, for their finding had even greater systematic meaning and stimulated fully as much research. It too perhaps is wrong.

What then about Franz and Lashley and some other contemporaries? Lashley owes a debt to Franz at least and has given us a great deal of new detail. But is his general finding something that marks an epoch? I have not space to attempt to draw the parallel between Flourens and Lashley but I think the fact that it can be drawn shows us something about originality in general. If there had been no intermediaries and if Lashley had come out with his general finding ninety years earlier, we should not have called him original and have

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[16]C. Broca, *Bull. de la Soc. anat.*, 2me sér., 6, 1861, 330-357.

[17]G. Fritsch and E. Hitzig, *Arch. f. Anat. u. Physiol.*, 1870, 300-332.

[18]Cf. S. I. Franz, *Science*, N. S., 35, 1912, 321-328; K. S. Lashley, *Psychobiol.*, 2, 1920, 55-128; *J. Comp. Psychol.*, 1, 1921, 453-468; *Amer. J. Physiol.*, 59, 1922, 44-67; *Brain*, 44, 1921, 255-285; *Arch. Neurol. and Psychiat.*, 12, 1924, 249-276; *etc.*

[19]On this history in general, see J. Soury, *Système nerveux central*, 1899, or his very long but briefer account in C. Richet's *Dictionnaire de physiologie*, II, 1897, 547-670.

dated the period from Flourens. As it is his work marks a reaction from the psychophysiology of 1870-1900 and is therefore more 'original.' Certainly one measure of originality is the degree with which a man can persist in the face of contrary opinion. The continuity of thought makes progress possible, but it works always against originality. There is no need to regret this conflict: Gall was original and should be respected for it, but only the gross underlying principle of phrenology contributed to progress. In general his originality impeded progress.

(*5*) *The speech center.* We may pause here to note that even Broca's discovery was not uncontested on the grounds of priority. Bouillaud in 1825 advanced the view on clinical evidence that the center for articulate speech lies in the anterior portion of the cerebral lobes.[20] He thought that he was thus supporting Gall and confuting Flourens. In 1836 Dax presented a similar view. In 1865, after Broca's discovery and after forty years of interest in other matters, Bouillaud in an elaborate paper challenged Broca's priority.

Who then discovered the speech center? I think it is plain that most of us will want to discount Bouillaud. There is no doubt about his priority, but priority is not after all what we are primarily interested in. It is not so much the first appearance of an idea that concerns us as its first appearance in such a way that it can and does develop into progress. (This same remark could have been made with less force of Bell and Müller and the specific energy of nerves.)

(*6*) *Reflex movement.* The importance attached to the reflex in modern psychology tends to direct attention to the history and discovery of reflex movement or action. Here, I believe, it is customary to cite Marshall Hall as the discoverer, although no behaviorist has attempted to 'canonize' him. Perhaps attention goes most naturally to Hall, because Johannes Müller, who claimed to have discovered reflex movement independently, nevertheless yielded priority to Hall.

Apparently the facts of some reflexes, without the name or theory, have long been known. Galen, for example, described what we now call the pupillary reflex. The word, and its attendant theory, seem to come from Anstruc (1736) in the sense of 'reflection' or 'reflected' movement. The theory was that, "as with light, angles of incidence and reflection are equal, so that a sensation produced by a concussion of the animal spirits against the fibrous columns [of the spinal cord]

[20] J. B. Bouillaud, *Arch. gén. de méd.*, 8, 1825, 25-45.

is reflected and causes motion in those nerve tubes which happen to be placed exactly in the line of reflection."[21] Whytt (1751) published important observations on this kind of movement in decerebrate animals.[22] Such observations, however, could not have the importance that would have been attached to them if the distinction between motor and sensory nerves and some idea of their connection had been known. Thus Marshall Hall's discovery of reflex movement in decapitated newts and snakes in 1832 immediately assimilated to itself a significance that Whytt's work could not have. Moreover, we see once again that the significant discovery may come easily as soon as the preparation is such as to make it significant. I mean here that it was the Bell-Magendie law more than anything else that gave physiological significance to the bare facts of 'reflected' movement. The Bell-Magendie law was not known internationally until the twenties. Within a decade both Hall and Müller 'discovered' reflected movement. Hall reported his study in 1832 and published in 1833.[23] Müller discussed reflected movement generally in 1833 and fully, claiming independence but yielding priority to Hall, in 1834.[24] It seems plain to me that this is another instance of originality depending upon scientific preparation.[25]

(7) *Hypnotism and Braidism.* We are usually told that the discoverer of hypnosis was James Braid. The facts that Braid coined the word "hypnotism" and that others called the phenomenon "Braidism" seem to support this view. Nevertheless it seems to me that there could hardly be any scientific subject-matter where it is more unfair to single out a discoverer. In spite of the violent controversy that this subject at least twice engendered, one may dare seventy-five years later to attempt to state the facts.[26]

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[21]Cf. C. F. Hodge, *Amer. J. Psychol.*, 3, 1890, 157.

[22]Cf. *The Works of Robert Whytt*, 1768. Since the completion of this MS. Carmichael has published on the work of Whytt, *Psychol. Rev.*, 34, 1927, 287-304. He shows more fully the significance of Whytt's work and gives some indication of other contemporaneous interest in the reflex. His conclusion, however, is consonant with mine.

[23]Marshall Hall, *Phil. Trans.*, 1833, 635-665.

[24]Joh. Müller, *Handbuch der Physiologie*, Bk. III, sect. iii, chap. iii (1834); Bk. IV, sect. ii, chap. i, 4 (1837).

[25]On the history of reflex action in general, see C. Eckhard, *Beiträge zur Anatomie und Physiologie*, 9, 1881, 29-192; G. S. Hall, *Amer. J. Psychol.*, 3, 1890, 71-86; C. F. Hodge, *ibid.*, 149-167, 343-363.

[26]There are various accounts of the history of hypnosis. A. Moll, *Der Hypnotismus*, 1889 and four subsequent eds. with Eng. trans. of all, is the most detailed, but lacks perspective. J. M. Bramwell, *Hypnotism, Its History, Practice, and Theory*, 1903, is the most relevant here. I forbear from giving the original references to Mesmer, Elliotson, and Esdaille. Bramwell and others give them. However, it is more with their activities than their writings that we are concerned.

The phenomenon itself has probably been known since antiquity in the sense that it has occurred in religious ecstasy and has perhaps been intentionally induced in savage ceremonials. Animal magnetism, as a peculiar natural force related to mineral magnetism, has been believed in by some educated people from at least the sixteenth century to the present day.

Mesmer, a Viennese physician, expounded a view of animal magnetism as explaining the influence of the stars upon human weal in 1766, and used magnets for curing disease. Later he met a Swiss priest who effected cures by a similar method but without the magnets. Mesmer abandoned the magnets. Had he not been capable of open-minded tolerance and of the scientific capacity to learn, he would have kept the magnets and have ignored the priest. Vienna was suspicious of Mesmer, so he removed to Paris, and developed there the famous *baquet* and all the tom-foolery and mystery that surrounded it. The soft music, the purple robe, Mesmer's manner and personality, the mysterious *baquet* itself, were all well designed to induce hypnosis, and its attendant phenomena must have been what Mesmer got. But the significance of these things could only have been half-conscious; Mesmer never knew why they worked nor had a theory of their operation better than the plausible theory of animal magnetism. No wonder when, at the height of Mesmer's tremendous popularity, the French government offered a reward to Mesmer for the disclosure of his secret, Mesmer "refused" to disclose it. He had no secret to disclose. The commissions who had been appointed to investigate the phenomena knew presumably as much about them as he. However, the methods resembled the hocus-pocus of charlatanry; Mesmer 'would' not divulge the 'secret;' he was denounced as an impostor, and, all his popularity gone, he withdrew from Paris to die presently in Switzerland. Ever since this time "mesmerism," as it soon came to be called, has been held in poor repute among most educated persons, and yet Mesmer discovered certain fundamentals about the method of inducing hypnosis. Is it because the name "hypnosis" had not been invented that he did not "discover" hypnosis, or is it rather for personal reasons that scientists will not recognize him now as then? Mesmer was carried away by his discovery and never relinquished a belief in his personal power. Hence the discovery fell just short of a scientific generalization, and Mesmer's egotism was distrusted.

In the early nineteenth century mesmerism became a show phenomenon for the stage (as it is now), thus falling still further into disrepute. In 1837, however, John Elliotson, Professor of Medicine at University College, a pioneer and a radical in medical science, a man of great imagination and of many sound ideas, the originator and accomplisher of the idea of the attachment of a hospital to a medical school, the arch-defender of the use of the stethoscope, so much despised by medical men at first, but a man whose very force and imagination gained him opposition and enemies—in 1837 this John Elliotson witnessed a demonstration of mesmerism and at once set about trying out its therapeutic usefulness in the hospital. He was soon convinced of its value. Had he had the wit to call it by some other name than 'mesmerism' and the tact to have proceeded slowly, he might have won the case for the new phenomenon. Elliotson, however, had too much force to be tactful, and in 1838 the Council of University College forbade the practice of "mesmerism or animal magnetism" in the hospital. Elliotson immediately resigned from the hospital and from the university. Then followed a long period of propaganda for new things in general and mesmerism in particular. Elliotson started and edited the *Zoist* as an organ for this movement. Later mesmeric hospitals sprang up all over Great Britain. But, although there was a limited circle of respectable believers, Elliotson in general lost friends, money, and reputation. The medical world rejected him completely. Yet did he not discover all of hypnosis except its name (unless of course it was already discovered by Mesmer)?

While all this was going on James Esdaille, having read of Elliotson's work, was actually using mesmerism as an anesthetic agent in India. Here he met with some scientific opposition, but also with some encouragement. He performed successfully hundreds of operations, a great many of them major, between 1845 and 1851, when he returned to England. At one time he had a hospital created for him by special funds. Later the Government took him back into its hospitals to work out the relation of mesmerism to medicine. Esdaille could not claim to be a discoverer. He admitted Elliotson's priority as to the method. He was the first person to use mesmerism extensively for anesthesia, but the first case was that of the amputation of a leg in England in 1842. Yet why do we never hear of Esdaille? Perhaps because the anesthetic properties of ether were discovered in 1846 and of chloroform in the following year, and mesmerism or

hypnosis did not therefore develop primarily as an anesthetic agent. But it is also to be observed that Esdaille, largely because he used the word 'mesmerism,' found himself in the disreputable camp of Elliotson.

James Braid antedated Esdaille but not Elliotson. In 1841 he undertook to expose a mesmeric performance on the stage, but, in attempting the exposure, was privately impressed by some of the phenomena. Then (like Mesmer and the magnets!) he remained open-minded and sought to experiment at home. He hypnotized his wife by having her gaze fixedly at the family sugar bowl. He found he could induce sleep in other members of his family and in his friends by having them fix their gaze upon an object. He elaborated a theory of visual fatigue and, only a few weeks after he had attempted the exposure of a 'charlatan,' appeared on the same stage to produce and to explain in physiological terms the same phenomena. In 1843 he published his book, *Neurypnology* (="neuro-hypnology," hence "hypnotize" and the other derivatives); and thus in brief hypnosis was 'discovered.'[27] Braidism grew, though it hardly flourished, was lost sight of, and was then rediscovered in France at Nancy and at the Salpêtrière, but with this later history we are not concerned.

Why was Braid the discoverer of hypnotism? Because mesmerism was rejected and Braidism was accepted, and Braid coined the word 'hypnotism.' And why was mesmerism rejected when the only real difference seems to have lain in the name? Because, I think, of the egotism of Mesmer and Elliotson, especially as they tended to assert peculiar power in themselves; because, in Elliotson's time, the name "mesmerism" was already established in disrepute; because Elliotson's vanity led him to oppose Braid and thus allied Braid with the medical profession; because Braid by temperament sought persistently to come at a 'normal' physiological explanation of the phenomenon even though his actual scientific advance was no greater than the advances of Mesmer, Elliotson, and Esdaille. In short personal values entered into the selection of a discoverer for hypnosis; but a careful scrutiny of history discloses mostly the continuity of scientific thought and only those small increments of originality that we have learned to expect.

*Other instances.* These examples from the history of physiological psychology are surely enough to establish my thesis that mutations

[27] James Braid, *Neurypnology, or, the Rationale of Nervous Sleep; Considered in Relation with Animal Magnetism*, 1843.

in scientific evolution are rare, perhaps almost unknown; that scientific thought, like individual thought, is sensibly continuous; and that creation or originality is not abrupt, nor mysterious. I may, however, mention a few other examples within psychology at large, which space would prevent including here even were further illustration desirable. The reader is free to solve these puzzles for himself, should he so desire.

(*8*) *Associationism.* Who founded associationism? The historical break is between Hartley and James Mill. Hartley, I think, is generally regarded as the founder, unless one insists on going back to the germ itself; then it is Locke, if one takes the first great influence, or Hobbes, if one keeps strictly to priority. It is hard for me not to regard Berkeley and Hume as associationists, though the principle does not seem so important in Locke.

(*9*) *Physiological psychology.* Who founded physiological psychology? Wundt, who insisted upon it and wrote under the title and is evidently both chronologically and psychologically at the beginning of some great new thing? Or Bain who put chapters on the nervous system into his psychology? Or Johannes Müller who put chapters on sensation and the mind into his physiology? Or Lotze who wrote a medical (and metaphysical!) psychology? Or Hartley with his vibratiuncles and speculation without experiment? Or Descartes, who was a physiological experimentalist and gave us our mind-body dichotomy?

Or, if physiological psychology is simply *experimental psychology*, Wundt or Fechner or the physiologists themselves, especially in the persons of Müller and Weber? Fechner was a philosopher by his own intention and ambition. He was an experimentalist in the interests of philosophy! If he founded experimental psychology, he did it inadvertently. Wundt was an experimentalist by intention and ambition, but a philosopher and systematist by temperament. Wundt meant to found experimental psychology, and it was founded. Did he do it? Fechner has the priority, but *he* wanted primarily to disprove materialism.

(*10*) *Context theory of meaning.* Who originated the context theory of meaning? Titchener, of course, if one intends the word "context" and the exact formulation that he attaches to it. Yet it is not clear whether, without losing significance, the theory can be divorced from association as a principle, and thus whether it differs greatly from some of the associational theories of meaning (or of the

object). And, apart from association, did not Berkeley essentially state this theory?[28] Did Titchener perhaps then simply adapt it to modern systematic uses?

(*11*) *Behaviorism.* Watson, I believe, claims to have founded behaviorism. Did he do more than add the 'ism' to behavior? Certainly the attempt to get along without consciousness in psychology is as old as the Cartesian divorce of the mind from the body. But others have raised this question and I do not need to extend my queries here.[29]

(*12*) *Gestalt psychology.* Who began *Gestalt* psychology?[30] Just at present in America the names of Köhler and Koffka are on everyone's lips. Both of these men refer the origination of *Gestalt* psychology to Wertheimer. Wertheimer modestly finds the roots in the school of *Gestaltqualität* and points to Von Ehrenfels. He in turn certainly owed something to Mach. But what is the cardinal principle of *Gestalt* psychology that is so all-important? Is it that the whole is always more than a mere *Und-verbindung* of all its parts (the argument against the atomism of associationism)? Is not this the view of the mind that James labored to convey?[31] Is *Gestalt* a protest against Wundt and his influence? What about Wundt's *Aktualitätstheorie*, his creative synthesis and the law of psychic resultants? Was Wundt nearly so 'atomistic' as his critics would have him be, or did he too have a vision similar to James', and Ehrenfels', and Wertheimer's, and try to picture it in a similar way?

## Conclusions

Scientific thought is a progressive unity. It evolves slowly, so slowly that the thought of an individual, incorporated within it, ordinarily does not represent a large increment of change. Nevertheless the rate of progress is very uneven and at times thought in a given subject-matter moves rapidly. Such an acceleration may be due to the unusual originality of a single individual, but it is more

[28] Cf., *e.g.*, George Berkeley, *Essay towards a New Theory of Vision*, 1709, par. 8-15, 17, 45-51, esp. 51. Also cf. Berkeley's par. 46 with the argument of E. B. Titchener, *A Beginner's Psychology*, 1915, 26-30.

[29] Cf., especially, C. M. Diserens, *Psychol. Rev.*, 32, 1925, 121-152, who, in discussing psychological objectivism, very clearly exhibits the continuity of thought from the Greeks to the present.

[30] In general, cf. M. W. Calkins, *Psychol. Rev.*, 33, 1926, 135-158; *Brit. J. Psychol.*, 16, 1926, 171-179.

[31] Cf., especially the chapter on the stream of thought, William James, *Principles of Psychology*, 1890, I, 224-290. Miss Calkins (*supra*) gives other references to James.

likely to be the result of previous converging tendencies which render a given 'discovery' the next natural step in the process. Many synchronous independent discoveries are thus to be explained.

It seems to me that scientific thought is thus like the personal thinking of the scientist. The scientist has his brilliant intuitions, but the originality of the moment of insight is little more than the unexpected seeing of a relationship. Both kinds of thought are evolutionary, and their stages are marked off by the anastomosis of logical processes. Only in this somewhat artificial sense are there mutations in the evolution of thought.

The essence of originality seems then to lie, not so much in sheer novelty, as in the systematic aspects of a discovery or a theory, that is to say, in the selection of data or conditions, their collocation, and the establishment or exhibition of the resultant relationships. The first result is a generalization. Thus Braid surpassed Elliotson, not as a discoverer, but because he gave a more generalized meaning to the hypnotic phenomena than the earlier assumption that they depended upon the animal magnetism of some undefined persons. But the broader generalization is systematization. The originality in the theory of the specific energy of nerves was little more than the collocation of thitherto unrelated facts.

Just as genius tends to be measured by reputation, so mere originality may pass unnoticed unless it yields certain results. Some, though not all, of the factors that bring originality into scientific attention are extrinsic to the individual concerned.

There is in the first place the 'doctrinization' of theories. Clear and emphatic exposition tends to turn theories into 'doctrines' or 'laws.' When others accomplish this process for an investigator, he is entitled to what credit it brings him. When the emphasis is his own, we must beware lest we confuse scientific acumen with expository or polemical skill. Here lie all the cases from the man with a persistent conviction based upon insight to the 'promoter' of a view who is merely defending his own 'greatness.'

In all these evaluations we must not lose sight of the fact that a man's departure from current tradition is a measure of himself and not of extraneous conditions inherent in the times. If something is lost from originality when it is seen to be but a step in a larger evolutionary process, then any step that is against the trend of science must gain in originality. Thus negativism is often originality, but we should not, I take it, wish to measure greatness by negativism.

The fact is that we try to measure the value of discoveries by their correctness. This practice is justified as against the results, but it seems to me but a poor measure of a man. Correctness is not only relative and a matter of the times, but it is thus entirely external to the investigator or theorist. The originality of a discovery or theory within a given universe is not lessened when the universe is enlarged, though its 'truth' may be. In fact many great discoveries are great because they led to beliefs in great falsities (as viewed later against a larger universe), which nevertheless in turn led on to new discoveries.

This fact leads us to note another measure of greatness which is largely external to the individual. Discoveries are important when they are significant, when they yield the broadest generalization or most numerous relationships. It may be true that it is the great man who perceives significance, but it is also true that the significance shows the problem to the 'great' man. This is why great discoveries do not occur until the times are ready for them or, when they occur too early, are not great. In other words the state of the times works both before and after the discovery. It prepares the way for the discovery and it develops it into further research and discovery after it is made. Thus the discovery may become a mere point in the evolutionary process and not such an important event after all.

There are of course great men, who as creative scientists greatly excel others, and who represent thus an individual difference that is a truly psychological difference. It is easy to name persons who certainly differ in scientific ability from Helmholtz. My thesis is that much of the greatness that is attributed to the 'great,' is an artifact and depends upon matters that are not intrinsic to the mind of the 'great' man. We can not always evaluate the intrinsic and the extrinsic factors in a given case, but we can at least realize the problem. Moreover we can realize how small is the thought of an individual in comparison with the process to which it belongs, and we can turn our attention to the larger and more important matter of the stream of scientific thought. Could we thus even partially dehumanize science, a great deal of fruitless and really meaningless controversy about both the past and the present would be avoided.

# WASHBURN'S MOTOR THEORY: A CONTRIBUTION TO FUNCTIONAL PSYCHOLOGY

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In her volume on movement and mental imagery[1] Professor Washburn has outlined a motor theory to account not only for the arousal of sensory and imaginal processes but for the "complexer life of the mind" as well. It is our contention that the efficacy of this theory lies not in explaining occurrence of and differentiation among 'images' and other 'mental' processes, but rather in the direction of suggesting variations in motor activity among what may be called the psychological operations or functions.

A reason for the change of emphasis from process to function is that the theory has more to do with ways of acting, of ideating, of thinking than with experienced qualities. We have only to read here and there in the text to realize this fact. As illustrative material we may cite the following explanatory statement. "The excuse which the present essay would offer for its own existence is that while the facts of attention, perception, and emotion have had their relation to bodily movement fully discussed, there still remain many phenomena connected with the complexer life of the mind, the revival of past experiences and the construction of new thoughts and ideas, whose connection with motor processes has not been satisfactorily traced."[2] What have we here but perceptive, emotive, memorial and elaborative modes of performance?[3]

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[1] M. F. Washburn, *Movement and Mental Imagery*, 1916.

[2] *Ibid.*, xiii. Other instances may be cited to show the permeation of the functional point of view. When speaking of the rôle of imagery in the reciting of a learned series of nonsense syllables Washburn suggests "If a hitch comes in the recitation, a mental image of the look of the syllables may be recalled" (32). Certainly the look of the syllables falls in the category of apprehension rather than of experience regarded process-wise. Furthermore, in treating the development of mental life from the animal to the human organism, Washburn speaks of two conditioning factors making for progress, greater discrimination among stimuli and the power of forming "free ideas" or the recalling of past experience. From the power of memorial recall develops ejective consciousness, a great step in mental growth. See article, A factor in mental development, *Phil. Rev.*, 13, 1904, 622–626. Here we may surely say that the development lies in greater discrimination by the apprehensive functions and in their socialization.

[3] As a matter of convenience we have employed Bentley's distinction of, and terminology for, the various psychological functions. See *The Field of Psychology*, 1925.

Washburn considers herself a 'structuralist.'[4] As we shall try to show, however, she appears to be far more functional than 'structural' in her general treatment of psychological problems.

For convenience we may divide the motor theory into two parts, which we shall designate as A and B. Part A has to do with the organization and grouping of movements into systems and sets during psychological functioning, and Part B with the relation of consciousness to movement. A logically precedes B; but we shall first consider B, since A is the basis of our major topic and B a secondary matter and may therefore be disposed of first.

The causative mechanism for the production of consciousness is the ratio of excitation to inhibition in a motor discharge. "If the amount of excitation either sinks below a certain minimum or rises above a certain maximum, consciousness is lessened."[5] Sensation and the mental image are products of this excitation-inhibition ratio, the one dependent on peripheral, the other on central (motor) excitation. "Whenever a motor pathway is at the same time excited by a sensory pathway and partially inhibited by an antagonistic motor excitation, a process occurs in all sensory pathways connected with the motor pathway that is exciting the motor pathway in question. This process is accompanied by consciousness" (30).[6] Centrally excited sensations are aroused not by association between one sensory pathway and another, but by induction into a motor pathway from a sensory center formerly excited by a peripheral source.[7]

Provided we limit the term 'consciousness' to the arousal of experience, to the qualitative interplay of sensory and imaginal components,[8] we can conceive of the validity of the ratio theory.[9] The

[4]"As a structural psychologist, then, I am prepared to defend the unity of the physical world." Introspection as an objective method, *Psychol. Rev.*, 29, 1922, 89.

[5]*Movement and Mental Imagery*, 25. Hereafter this volume will be referred to by page-numbers set in the text.

[6]See also Washburn, The function of incipient motor processes, *Psychol. Rev.*, 21, 1914, 382.

[7]Two facts from Sherrington's neurology were adopted to aid in the formulation of this hypothesis: (1) antagonistic muscles contract and relax simultaneously in a reciprocal manner and in varying degrees, and (2) afferent neurones in the central nervous system of vertebrates form no direct lateral connections with one another.

[8]Affection as a component is not referred to in the text.

[9]Washburn does not make this limitation. She frequently employs the term 'consciousness' more broadly and promiscuously, e.g. *Animal Mind*, 354. In consciousness there may be "revival of ideas and the development of a plan."

proportion between excitation and inhibition in motor pathways may appear as a factor in sustaining experience. As movements become freer and less hesitant experience diminishes. A good performer of the touch system in typing experiences fewer visual, auditory and kinaesthetic complexes of qualities than does the novice. On the other hand, progressive relaxation of groups of muscles decreases the complexity and richness of experience.[10]

Two chief reasons, however, exclude the possibility of the innervation-ratio theory from being all-inclusive for the arousal of experience. In the first place, without bringing in the brain as *deus ex machina* the theory is sieve-like. At the spinal level a ratio between muscular excitation and inhibition might be maintained without any experience taking place. What is the elusive factor within the brain that turns the trick?

Secondly, in the light of present-day neurology, it seems that the cerebrum, and, in fact, the entire nervous system, functions in an integrative manner, not merely as a congeries of isolated pathways. Integration and unity of function, therefore, have to be taken into account as a part of the bodily setting for experience.

We may conclude, then, that the provocative-inhibitive ratio of activity in motor pathways may be at most one of the contributing factors to, rather than the sole cause of, the appearance of experiential qualities.

Part A of the motor theory is less speculative than B. We believe that it adds to our knowledge of functional psychology. We especially refer to that part of the theory which deals with movements, their grouping, organization and variation in the living organism while it thinks, desires, searches, works, acquires knowledge, appreciates beauty or laughs at a jest.

According to Washburn, psychological functions develop, become differentiated, and increase in complexity through the basis of associatory movements. Association occurs in one of two ways; either where certain movements drop out or where there is preservation of movement with organization into systems. In the first way, that of the conditioned reflex, stimulus A, originally producing the response A, may acquire the ability to produce the response B and to lose its own response. In the other way, stimulus A not only produces the response B but keeps its own response as well.

These associated movements are called movement-systems and are defined more specifically as "a combination of movements so linked together that the stimulus furnished by the actual performance of certain movements is required

[10]E. Jacobsen, Progressive relaxation, *Amer. J. Psychol.*, 36, 1925, 73–87.

to bring about other movements" (8). Two types of movement-systems are distinguished, the static and the phasic. The static include those movements in which prolonged simultaneous contraction of some muscles and relaxation of others take place, as in maintaining somatic postures. The phasic system requires "actual translation in space, or movements as we commonly understand the term" (11). The contraction of the muscles in the second group need not be necessarily simultaneous, but may be successive, a chain-like process, as in the recitation of a series of nonsense syllables. "The performance of a particular muscular contraction furnishes the stimulus for one other only which accordingly follows it" (12).

Besides movement-systems there are sets. A set of movements comprises "a number of movements whose only relation to each other is that they are all associated with one common movement" (16).

A further differentiation of muscular organization is that of fullness and tentativeness. Tentative movements are the slight, incipient ones that accompany attention to the stimulus or which go on in "silent thinking or reading." They involve fewer neurones than do full movements and are more economical of energy. The rôle played by these labor-saving devices is so important in psychological functioning that it might be well to pause here a moment for a cursory glance at their evolutionary development.

Washburn shows how tentative movements most likely first came about in animals with distance receptors. Immediate reaction was not necessary in responding to visual, auditory or olfactory stimulation. The nervous system developed the means of temporarily suspending immediate full response to a stimulus and substituted mediate, partial or tentative response.[11]

Another aid in bringing about refinement of tentative movements was a greater number and variety of movement-systems. Full and tentative movements cannot both be made simultaneously with the same set of muscles. The vocal apparatus is of course the most fertile place for those tentative movements which may be initiated without interference with the locomotor or manipulative muscular systems.

Movements that interfere with one another are called incompatible, as the simultaneous raising and lowering of a limb or the concurrent pronouncing of the letters *d* and *f*. On the other hand, lifting the arm and uttering *d* at the same time are within the scope of compatibility.

In addition to this check on unlimited production of movement, Washburn mentions prepotency as an inhibitory influence of one set or system of movements on another. One movement is made preferably and prior to another if it is compulsory through racial setting, through needs of the organism or through acquired training.

All the various movement incorporations, temporal and spatial, occur not only, as we might expect, in the executive functions but in the other psychological operations as well. In all of them there is some sort of motor response, whether it be gross bodily activity or the more refined tentative incipiency in the speech mechanisms.

[11] *The Animal Mind*, 3rd ed., 1926. Esp. chap. xiii.

But first of all we must mention certain antecedents of function which are characterized by Washburn as the "set in readiness," the associative tendencies and the determining tendencies. If a motor response is very slight, not enough to produce consciousness, nevertheless the effects of the excitation are revealed in the phenomenon of readiness to apprehend or to act.

One form of readiness occurs with the memorial after-image which is based on the "tendency of movements, whether full or tentative, to repeat themselves spontaneously just after they have been performed" (83).

This readiness for spontaneous activity in a previously excited motor process may extend beyond the duration of the memory after-image. An instance from the laboratory may serve as an illustration. The observer is told that when he sees a green light he is to press a key with his right hand, when he sees a blue light he is to press a key with his left. He repeats the instructions to himself; *i.e.* he attends to each of the alternatives by making such tentative movements as the sub-vocal "green-right," "blue-left." If the observer has to wait long he may repeat the tentative movements. In any case there is readiness to execute the act one way or the other.

Thirdly, the readiness of the nervous system to motor discharge happens in instances free from any imagery but simply through the means of "peripherally excited kinaesthetic sensations." Washburn cites "sea-legs" and persistence of organic attitudes as exemplifying.

In the fourth place, readiness in an exaggerated form is perseveration, the persistence which takes place generally in fatigue and in abnormal states.

The tendencies within the nervous system for associative dispositions to be formed mark the second type of antecedent of function. This proclivity is responsible for the association between movements, their formation into simultaneous and successive sets and systems. The neural condition is supposedly that of "lowered synaptic resistances between the kinaesthetic centres excited by the performance, either tentative or full, of one movement and the motor centre belonging to another movement" (152).

Lastly, we find in the determining tendencies a type of predisposition more persistent in its effect than associative tendencies and more directive than perserveration. "An associative tendency becomes a determining tendency through the operation of some factor that is not itself an ordinary associative tendency" (155). This factor seems to be a system of static innervations, steady and persistent within the organism, which is in reality a static movement system and which may be called the "activity attitude." In a determining tendency a motor associative disposition becomes associated with an internal static movement system.

In a piece of work not specifically directed toward the classifying, identifying and discussing of psychological functions, it is not always possible to separate these matters; to say that here we have memory, there understanding, or again thinking. The task is pretty much the same as when we attempt to make an arbitrary functional differentia-

tion of our everyday activity in a given period of time. Within limits, however, we have found a description of the movements involved more or less uniquely in most of the psychological functions. We shall first deal with the more primitive modes; namely, action and apprehension.

Because all psychological activity is, according to Washburn's theory, founded on movements, it is only logical for us to consider first the function of action. In a smooth-running automatic or habitual action, the motor response is fully initiated and entirely unopposed. It is the "performance of full motor response without delay and without consciousness" (36). In delayed action, however, a true psychological function makes its appearance. For only in delay can an action-problem be formulated; can the realization and anticipation of something to be done materialize. Delayed actions came about when the animal could first make tentative movements; *i.e.* try out a movement before bringing it to completion. In fact, according to Washburn, this is their original function. Now the production of tentative movements, of sampling a performance before its execution, has led to development along two lines; namely, to more skill within the same function and to the apprehensive functions. Before performing complicated movements requiring agility, it is first necessary to attend to the kinaesthetic excitations resulting from the tentative movements so as to be sure the stage is set for just the adequate adjustment of muscles. On the other hand, delay leads to the arousal of apprehension.

The crudest manifestation of an apprehensive function is where a motor response is initiated but no actual performance of a movement, either tentative or full, occurs. "This is accompanied by 'consciousness of the stimulus;'[12] but no associative activity can take place because associative activity requires the actual performance of a motor response to produce the kinaesthetic excitations necessary to set other motor pathways into action" (36).

For convenience we shall call this elementary form bare perception. More elaborate apprehension comes about when a motor response is still more strongly initiated. Tentative movements act "slowly and with delays." 'Consciousness' and associative activity

[12]By 'consciousness of the stimulus' Washburn here refers to a sensation that suggests nothing associative. In other places in the text, sensations capable of suggesting associations are considered. (See esp. 58). The term is therefore confusing.

are both present. In bare perception experience is meager and limited. In the more complex perceptions and in memory and imagination, experience is richer, more extensive and more highly organized into sensory and imaginal integrations.

As we have seen previously, the distinction between sensory and imaginal integrations, between sensations and images, lies in the fact that, in the former, the original stimulus is an outside physical force that initiates the motor response, whereas in the latter the stimulus instigating the motor response is another movement.[13] Another differentiation between these two types of organization lies in their duration. Sensory integrations are more persistent, imaginal more fleeting, due again to the nature of their initial stimulation. Stimulation from bodily movement is capable of only brief duration.

Washburn very rightly does not make the origin of stimulation a distinguishing feature of perception as opposed to memory and imagination. The more elaborate a perception, the more is it enriched by imaginal components. "The perception of an object consists of a number of sensations, some of which are peripherally excited, that is, caused by the activity of our sense-organs at the moment, while others are centrally excited, that is, the revival of former sense experiences. A piece of ice looks smooth, white, hard, and cold. We realize that only the whiteness of it is the result of present stimulation, and that the hardness, smoothness, and coldness are the effects of former experiences with the senses of touch and temperature" (130).

Furthermore, the greater the intensity of memory and imagination the more are they accompanied by diffuse and extensive sensory ingredients. "I can imagine a very loud sound, and when I do so, I feel certain sensations of shrinking and shock which are evidently the result of movements produced because they are associated with the motor processes involved in the idea 'very loud sound' "(39).

Intensity of the general motor disturbance, however, is a point of differentiation among the apprehensive functions. In perception this extensive motor activity is felt "sometimes as fused with the sensation proper to the stimulus, if the kinaesthetic and organic sensory excitations which it sets up do not produce a special motor reaction of their own; sometimes it is attended to on its own account

[13]Washburn's images are not imaginal components in the strict sense of being organized only centrally. They are rather an integration of peripheral (bodily movement) and central (inductive) conditions.

and more or less analyzed into localized components, whether analyzed or unanalyzed. The kinaesthetic excitations coming from this general motor disturbance are probably the basis of a characteristic difference between the sensation from an outside stimulus and the recalled image" (38).[14] A similar motor response may be part and parcel of remembering and imagining, as in the instance we have cited of imagining a loud sound; but while "the intensity of a mental image consists of the kinaesthetic, organic feel of a general motor response, the 'feels' of which image intensities consist are not identical with those which sensation intensities consist" (40).[15]

Let us consider for a moment whether the characterization by intensity can be justified. Are there not occasions when the general motor disturbance resulting in kinaesthetic-organic complexes of experience are far greater and more extensive in imagination or in memorial recall than they are in perceiving? Think of apprehending the presence of a pin on the floor as compared to the recalling of a recent embarrassing predicament!

A second dissimilarity among apprehensive functions consists in the number and extent of movements contained in memory and in imagination. In a research on imagination Perky discovered that her observers employed "more eye-movements and more kinaesthesis generally in memory images than in imagination images."[16] Furthermore, "in imagination consciousness is narrowed and there is inhibition of irrelevant associations, while in memory attention wanders and the image is unstable."[17] Washburn accepts these results as fitting into her system for she says "if an observer is required to call up, besides what we should call the memory image of an object, memory images of its surroundings and attendant circumstances on a particular occasion, the motor systems will naturally be more complicated than when the object is recalled alone" (147).

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[14]Here the terms 'sensation' and 'recalled image' refer very definitely to functions.

[15]We might argue that the intensive factor is a quantitative difference between sensory and imaginal components as duration was found to be. Duration, however, refers for our author to both sensory and imaginal qualities of experience *per se*, whereas intensity refers to the kinaesthetic and organic feel (sensory qualities only) of general motor responses, etc.

[16]C. W. Perky, An experimental study of imagination, *Amer. J. Psychol.*, 21, 1910, 422-452.

[17]Memory, in this case, is taken in the sense employed by Bentley, as the apprehending of an object with a dated pastness. Washburn defines memory as the recalling or imagining of any known object, and imagination as the forming of new combinations out of past experiences. "One may imagine a centaur, but one has to remember a horse" (147).

The distinction seems to be one of particularization, for the reverse state of affairs is conceivable where a more extensive series of movements is initiated in imagining rather than in remembering. As an illustration, consider the multiplicity and extent of the tentative and full movements both of speech and of bodily attitudes assumed in imagining the delivery of a significant speech at a forthcoming banquet, as opposed to the simplicity of the movements introduced in remembering whether one dined last night on steak or chicken.[18]

On the other hand, two points of similarity among apprehensive functions are indicated by Washburn. Both of these refer to perception and to recall. The first common factor is the presence of organic processes in their qualitative rather than their quantitative aspect.[19] "The general organic response to a stimulus furnishes masses of kinaesthetic excitation which we cannot analyze in many cases but which are probably highly varied and capable of representing, in movement systems, the different individualities of many stimuli and combinations of stimuli" and "the peculiar motor effect of a stimulus on the organism as a whole may be the basis of the recall of mental imagery" (47).

The other likeness between perception and recall is in the strength of old associative dispositions. A fundamental law of perception exists in the fact that "whenever in a movement system it is possible for an old associative disposition, based on much repetition, to take the place of a new one whose strength lies rather in recency than in repetition, the substitution occurs" (147). This law explains the illusory perception of two objects when a pencil is held between two crossed fingers. The tentative movements belonging to two pencils are set up as a result of tactual stimulation even though a unitary response to the visual stimulation is made. The associative dispositions stirred up by the "sight of the single pencil are not able to withstand the old dispositions connected for life with the stimulation of two points on the skin that are not normally reached by a single object at the same time" (148).

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[18]The conclusions of Perky have been questioned. See H. Clark, Visual imagery and attention; an analytical study, *Amer. J. Psychol.*, 27, 1916, 461-492. According to Clark the presence and frequency of ocular movements during imagining have "no immediate and intrinsic connection either with familiarity or with 'personal reference' and 'particularity,'" but have more to do with the general conditions of attention.

[19]As we have seen, the intensity of the bodily processes resulting from general motor disturbance is a point of difference rather than of similarity.

For recall the same law holds. Memory images tend to alter in the direction of the usual and normal, losing their particular and specifying features.[20]

We may conclude, therefore, that, except for the difference between bare apprehension and more elaborate forms, the motor theory gives us no all-inclusive points of demarcation among the functions of perceiving, remembering and imagining. It indicates certain distinctions but these break down when taken out of their specific context. On the other hand, the theory suggests certain similarities that seem to hold true, *i.e.* the prepotency of old associative movement-systems and the rôle played by various kinaesthetic-organic integrations in lending particularity and individuality to perception and to recall.

If we turn to emotion we find it treated in the chapter on 'dissociation.' An emotion prevents movement systems which should normally connect themselves by associative dispositions from entering into a common system. One reason for the production of dissociation by an emotion is that the latter consists of a "movement system of the greatest complexity," involving practically every muscle of the organism. "Every bodily movement and every idea feels its effect either in the way of excitation or inhibition" (222).[21]

Another cause of the dissociating effect of an emotion, more particularly one of an unpleasant nature, is that "if a movement already belongs to a strongly formed movement system, it will enter with great difficulty into any system that contains incompatible movements with those of the first system" (224).

A similar explanation holds true for moods, the more persistent emotive attitudes. These more or less enduring despondencies, joys, sulks and fears consist of actual static movement-systems embracing certain innervations. While they are maintained they will inhibit the production of all incompatible reactions.

[20]Recall does not invariably refer in Washburn to the memorial function in the strict sense.

[21]In a recent address (not yet published) on *Emotion and thought, a motor theory of their relation*, Washburn distinguishes two types of movements that may enter into an emotional complex. In the first place, she speaks of the adaptive movements of the grosser striped muscles (of the limbs, *e.g.* in running away from a preying enemy). These movements are of definite demarcation. The second group consists of the non-adaptive movements of the striped muscles, often covering a wide area of the body; *e.g.* the motor explosion in anger. These movements are more diffuse and less definitely localized than are those of the first group. (These are not the only somatic reactions occurring in emotion. A third group of reactions according to Washburn are those of the viscera, depending on glandular and secretory systems.)

The postures (relational or imageless processes), those quick turns of doubt, of certainty, of 'but,' and of 'if,' that indicate "from moment to moment the position of the organism upon matters at issue"[22] fit very nicely into the motor theory. Being gauges of the temporary conditions of the total organism, just as moods are gauges of more permanent conditions, they may well be treated within the general heading of emotivity. The postures have on the motor side certain "definite and characteristic systems or attitudes." Their sudden flashing-on-the-screen is due, for Washburn, to a rapid shift of motor excitation from one response to another. On the side of experience, they appear as fusions, greater or less, of kinaesthetic excitations. "When, in the interplay of movement systems which is constantly going on, certain emergencies arise, certain characteristic motor attitudes are produced. Some of these have a tendency to develop and diffuse themselves until they produce in consciousness analyzable organic attitudes like the emotion of surprise or the conscious attitude of doubt; others have not this expansive tendency" (199).

When we come to consider what Washburn has to say about the "complexer processes of the mind" we find reference frequently made to some of the more complicated psychological functions, of understanding, of solving problems, and of reaching new conclusions.

In the first place, Washburn introduces the concept of the problem-idea, a mechanism that may occur in several of the psychological modes of operation. The problem-idea (purpose, resolve) initiates and sustains prolonged action, the seeking to comprehend or the thinking through. Its characteristic feature is the "persistence of its influence." In colloquial terms, when the 'mind' wanders, the problem-idea does not exist, when the 'mind' is intent on doing something, or planning or thinking, it is present.

The problem-idea starts a movement-system into action, and one so complicated that the action is not worked out in a brief space of time. "The activity of such a system has a strong tendency to recur" (154).

An idea is transformed into a problem-idea through the influence of determining tendencies. These, as we have already seen, are

[22]Bentley, *Field of Psychology*, 357.

associative dispositions reënforced by the activity attitude, an internal static movement-system due to a persistent set of innervations.[23] On the inspective side the activity attitude is described as the feeling of effort.

The feeling of striving, however, is only one of two essential features of the problem-idea. The other is the felt uneasiness or restlessness. This restlessness results from "diffused and shifting motor innervations, which apparently have no useful connection with each other, and seem rather to be the effects of a common cause than to form a true system" (161). Only when both attitudes of activity and of shifting are sustained will the rat get out of the maze, the instructor hand in the semester's report-cards, or the astronomer discover the displacement of a star-image within the gravitational field of the sun.

Washburn mentions two forms, judging and reasoning, that the processes of directed thinking may take.[24] In the act of judging, two objects, events, ideas or concepts (as A and B) are not merely associated; rather A is B. "When A suggests B, either the two movement systems, A and B, form part of one and the same system, so that there exist associative dispositions between them as wholes, or some movement or smaller movement system contained in A is identical with a movement or smaller movement system contained in B" (175). But when it comes to the formula A is B, the A does not lose its significance. "A part of the movement system concerned with the subject lasts over after the rest has ceased to act. . . . This smaller system, a component part of the subject system, is the predicate system. Its emergence and persistence is [are] followed by a recurrence of the subject system as a whole. Thus, the sequence is A–B–B-as-a-part-of-A. And the subsequent associative dispositions will be determined not only by the predicate B, but by the predicate in connection with the subject A" (176).

The judgments made by individuals in the course of their daily lives are determined according to Washburn by their problems. By

[23]See the address cited in footnote 21. Washburn there states that the movements underlying the activity attitude are largely those of the muscles of the trunk, while the movements underlying the solving of the problem are those of the finer muscles, of the eyes, of the fingers, and especially of the larynx.

[24]Undirected thinking refers to the function of imagination, of revery, where the activity attitude is lacking. See especially Washburn's *Emotion and Thought.*

problems are meant the particular topics in which the individual is interested. The farmer will pass judgment on the desirability of a piece of land for planting his crops, the archaeologist will judge whether the same piece of land looks promising for excavating purposes.

Reasoning, on the other hand, is an indirect judgment embodying the influence of a problem-idea and more particularly of the activity attitude. In this case, A and B, subject and predicate, act together as a problem-idea. Two systems of tentative movements, A and B, operate in order to reach a third movement system, C, compatible with A and B. The solution of this thought-problem is "the setting into action of movement systems that are connected with both of these systems; any other movement system will be inhibited by the persistent recurrence of these two as the problem-idea" (177). The new movement system C connected with the two older ones, A and B, has to be a simultaneous system and a "simultaneous system cannot contain incompatible movements." Thus, in order really to think through a problem, a new simultaneous movement has to be found.

If we ask the question 'Is this room too hot to work in?' and reply 'Yes; because we were told so by Mr. X,' we have an instance of fallacious rather than of real reasoning for there is still room for a movement system as 'Is Mr. X reliable in his statements?' Such a statement is obviously incompatible with the already formed decision. Not until some member of the full system A (this-room-now), a very complicated system, proves to have associative dispositions in common with some member of the full system B (too-hot-to-work) will the real solution be reached.

In fallacious reasoning older dispositions and systems prevent the forming of new ones. That is "old prejudices and even mere verbal associations exert their contaminating effect" (179). In creative thinking, a new system is formed. "As the various component parts of the two problem systems come into especial activity, they find various common motor outlets, the tentative hypotheses or suggestions towards the solution of the problem. But each of these must be inhibited while the other parts of the problem systems are allowed to come into activity, until a direction of motor discharge is found which is common to them all, and a solution is reached which leaves no part of the problem out of account" (183).

Our general contention that Washburn writes in terms of function rather than structure holds true particularly in the case of judging and of reasoning. Judging is a mode of operation. It may occur at the perceptive level; *i.e.* when we see that "the chandelier is swinging" (176). It may also go on at the comprehensive level; *i.e.* when a painter "on going into a room makes judgments about the pictures on the walls" or the "electrician makes judgments about its lighting arrangements" (177).

Reasoning is also a type of performance of the psychological organism. In reasoning out the solution of a mathematical equation we do not merely experience process-wise; we also act function-wise. True reasoning seems to lie within the operation of thinking-through and of arriving-at new conclusions by the medium of symbolic meanings. False reasoning, on the other hand, falls in the realm of understanding, of thinking about matters in the light of already formed notions and opinions.

Beside questioning Washburn's use of the term "processes of thinking," we are doubtful about the advisability of employing the words 'judging' and 'reasoning' themselves. Washburn has here taken her terminology directly from logic and what she does is to speak primarily of logical and only secondarily of psychological functions.[25] Logical functions are formal, limited, and arbitrarily set; whereas psychological functions are more flexible and of greater variety. Judging and reasoning, therefore, do not give us a complete picture of the various ways in which the total organism functions above the apprehensive level. In fact, it is doubtful whether the terms signify much from the psychological point of view; rather are they logical means of treating major and minor premises, of combining subjects and predicates.[26]

In summarizing our discussion of Washburn's motor psychology we may say that the theory, as we understand it, offers an hypothesis as to the rôle played by the musculature in the various psychological functions. The whole thing is a very clever and logical presentation of the way in which the muscles are organized and integrated into

[25]Especially is this true in Washburn's elaboration of the various forms of illicit reasoning. See *Movement and Mental Imagery*, 180 ff.

[26]Sometimes in the text we come upon the term 'creative thinking' as a substitute for reasoning. This term it seems to us is far more expressive of a functional category.

systems and sets, remain static under enduring trends, change and shifts in varying phases, form new combinations and incorporate them into older existing units.

An added advantage in favor of the theory is the number of ways it can be verified empirically and experimentally. Movements can be observed with the naked eye from one individual to another, as in the lifting of an arm, the running for a street car, or the trembling in fear. Movements can be recorded on instruments, pneumographs, ergographs, planchettes and devices for lingual and laryngeal registration. A person may comment on his own reactions as "I am on the verge of laughing," or "when reading to myself I make little twitches in the throat." Or, fourthly, introspection may be in order. The observer describes his kinaesthetic and organic complexes, as well as the extent, location, quality and intensity of the numbers. And, furthermore, as more experimental evidence pours in,[27] the motor theory may stand on still firmer ground.

But the crucial point, as we have attempted to show, lies in the fact that when Washburn refers to the processes of the 'mind' she does not mean psychological experience taken 'structurally' but rather 'functionally.' When she speaks of sensations of objects, of mental images, of emotions, of the problem-idea as a sustained state of consciousness, of judging and of reasoning as the processes of thinking, Washburn has left the realm of 'structural' description and has entered the field of the functional psychologist whose task it is to describe the modes of performance of the total organism.

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[27] *E.g.* as more delicate and elaborate apparatus is devised for hearing the faint contractions of the finer muscles, or as more accurate description of the quality, localization and extent of kinaesthetic and organic patterns accrues.

# THE JAMES-LANGE THEORY OF EMOTIONS: A CRITICAL EXAMINATION AND AN ALTERNATIVE THEORY*

By WALTER B. CANNON, Harvard University

In his introduction to the reprinting of the classic papers by James and Lange, Dunlap[1] declares that their theory of emotions as organic processes "has not only become so strongly entrenched in scientific thought that it is practically assumed today as the basis for the study of the emotional life, but has also led to the development of the hypothesis of reaction or response as the basis of all mental life." And Perry[2] has written, "This famous doctrine is so strongly fortified by proof and so repeatedly confirmed by experience that it cannot be denied substantial truth. In spite of elaborate refutation it shows no signs of obsolescence." With some trepidation, therefore, one ventures to criticise a view of the nature of emotions which has proved so satisfactory as a means of interpreting affective experience and which has commended itself so generally to psychologists. There are now at hand, however, pertinent physiological facts which were not available when James and Lange developed their ideas and which should be brought to bear on those ideas, and there are alternative explanations of affective experience which should be considered, before the James-Lange theory is granted basal claims in this realm of psychology.

James first presented his view in 1884, Lange's monograph appeared in Danish in 1885. The cardinal points in their respective ideas of the nature of emotions are so well known that for purposes of comment only brief references need be made to them. James' theory may be summarized, in nearly his own terms, as follows. An object stimulates one or more sense organs; afferent impulses pass to the cortex and the object is perceived; thereupon currents run down to muscles and viscera and alter them in complex ways; afferent impulses from these disturbed organs course back to the cortex and when there perceived transform the "object-simply-apprehended" to the "object-emotionally-felt." In other words, "the feeling of the bodily changes as they occur is the emotion—the common sensational,

*This paper is from the Laboratory of Physiology, Harvard Medical School.
[1]W. James and C. G. Lange, *The Emotions*, 1922.
[2]R. B. Perry, *General Theory of Value*, 1926, 295.

associational and motor elements explain all."[3] The main evidence cited for the theory is that we are aware of the tensions, throbs, flushes, pangs, suffocations—we feel them, indeed, the moment they occur—and that if we should take away from the picture of a fancied emotion these bodily symptoms, nothing would be left.

According to Lange[4] stimulation of the vasomotor center is "the root of the causes of the affections, however else they may be constituted." "We owe all the emotional side of our mental life," he wrote, "our joys and sorrows, our happy and unhappy hours, to our vasomotor system. If the impressions which fall upon our senses did not possess the power of stimulating it, we would wander through life unsympathetic and passionless, all impressions of the outer world would only enrich our experience, increase our knowledge, but would arouse neither joy nor anger, would give us neither care nor fear." Since we are unable to differentiate subjectively between feelings of a central and peripheral origin, subjective evidence is unreliable. But because wine, certain mushrooms, hashish, opium, a cold shower, and other agencies cause physiological effects which are accompanied by altered states of feeling, and because abstraction of the bodily manifestations from a frightened individual leaves nothing of his fear, the emotion is only a perception of changes in the body. It is clear that Lange had the same conception as James, but elaborated it on a much narrower basis—on changes in the circulatory system alone.

## A Consideration of the Visceral Factors

The backflow of impulses from the periphery, on which James relied to account for the richness and variety of emotional feeling, was assumed to arise from all parts of the organism, from the muscles and skin as well as the viscera. To the latter, however, he inclined to attribute the major rôle—on "the visceral and organic part of the expression," he wrote, "it is probable that the chief part of the felt emotion depends."[5] We may distinguish, therefore, his two sources of the afferent stream. We shall first consider critically the visceral source. In connection therewith we shall comment on Lange's idea that the vasomotor center holds the explanation of emotional experience.

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[3]James, *op. cit.*, 123. [4]Lange, *op. cit.*, 73. [5]James, *op. cit.*, 116

(*1*) *Total separation of the viscera from the central nervous system does not alter emotional behavior.* Sherrington[6] transected the spinal cord and the vagus nerves of dogs so as to destroy any connection of the brain with the heart, the lungs, the stomach and the bowels, the spleen, the liver and other abdominal organs—indeed, to isolate all the structures in which formerly feelings were supposed to reside. Recently Cannon, Lewis and Britton[7] have succeeded in keeping cats in a healthy state for many months after removal of the entire sympathetic division of the autonomic system, the division which operates in great excitement. Thus all vascular reactions controlled by the vasomotor center were abolished; secretion from the adrenal medulla could no longer be evoked; the action of the stomach and intestines could not be inhibited, the hairs could not be erected, and the liver could not be called upon to liberate sugar into the blood stream. These extensively disturbing operations had little if any effect on the emotional responses of the animals. In one of Sherrington's dogs, having a "markedly emotional temperament," the surgical reduction of the sensory field caused no obvious change in her emotional behavior; "her anger, her joy, her disgust, and when provocation arose, her fear, remained as evident as ever." And in the sympathectomized cats all superficial signs of rage were manifested in the presence of a barking dog—hissing, growling, retraction of the ears, showing of the teeth, lifting of the paw to strike—*except* erection of the hairs. Both sets of animals behaved with full emotional expression in all the organs still connected with the brain; the only failure was in organs disconnected. The absence of reverberation from the viscera did not alter in any respect the appropriate emotional display; its only abbreviation was surgical.

As Sherrington has remarked, with reference to his head-and-shoulder dogs, it is difficult to think that the perception initiating the wrathful expression should bring in sequel angry conduct and yet have been impotent to produce "angry feeling."

At this point interpretations differ. Angell[8] has argued that Sherrington's experiments afford no evidence that visceral sensation plays no part in the emotional psychosis, and further that they do not

[6] C. S. Sherrington, Experiments on the value of vascular and visceral factors for the genesis of emotion, *Proc. Roy. Soc.*, 66, 1900, 397.

[7] W. B. Cannon, J. T. Lewis and S. W. Britton, The dispensability of the sympathetic division of the autonomic system, *Boston Med. and Surg. J.*, 197, 1927, 514.

[8] J. R. Angell, A reconsideration of James's theory of emotion in the light of recent criticisms, *Psychol. Rev.*, 23, 1916, 259.

prove that the psychic state, "emotion," precedes its "expression." And Perry[9] has declared that whether in the absence of sensations from the organs surgically isolated, the emotion is *felt* remains quite undecided.

It must be admitted, of course, that we have no real basis for either affirming or denying the presence of "felt emotion" in these reduced animals. We have a basis, however, for judging their relation to the James-Lange theory. James attributed the chief part of the felt emotion to sensations from the viscera, Lange attributed it wholly to sensations from the circulatory system. Both affirmed that if these organic sensations are removed *imaginatively* from an emotional experience nothing is left. Sherrington and Cannon and his collaborators varied this procedure by removing the sensations *surgically*. In their animals all visceral disturbances through sympathetic channels—the channels for nervous discharge in great excitement—were abolished. The possibility of return impulses by these channels, and in Sherrington's animals by vagus channels as well, were likewise abolished. According to James's statement of the theory the felt emotion should have very largely disappeared, and according to Lange's statement it should have wholly disappeared (without stimulation of our vasomotor system, it will be recalled, impressions of the outer world "would arouse neither joy nor anger, would give us neither care nor fear"). The animals *acted*, however, insofar as nervous connections permitted, with no lessening of the intensity of emotional display. In other words, operations which, in terms of the theory, largely or completely destroy emotional feeling, nevertheless leave the animals behaving as angrily, as joyfully, as fearfully as ever.

(*2*) *The same visceral changes occur in very different emotional states and in non-emotional states.* The preganglionic fibers of the sympathetic division of the autonomic system are so related to the outlying neurones that the resulting innervation of smooth muscles and glands throughout the body is not particular but diffuse.[10] At the same time with the diffuse emission of sympathetic impulses adrenin is poured into the blood. Since it is thereby generally distributed to all parts and has the same effects as the sympathetic impulses wherever it acts, the humoral and the neural agents cooperate in producing diffuse effects. In consequence of these arrange-

[9]Perry, *op. cit.*, 298.
[10]Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage*, 1915, 26.

ments the sympathetic system goes into action as a unit—there may be minor variations as, for example, the presence or absence of sweating, but in the main features integration is characteristic.

The visceral changes wrought by sympathetic stimulation may be listed as follows: acceleration of the heart, contraction of arterioles, dilatation of bronchioles, increase of blood sugar, inhibition of activity of the digestive glands, inhibition of gastro-intestinal peristalsis, sweating, discharge of adrenin, widening of the pupils and erection of hairs. These changes are seen in great excitement under any circumstances. They occur in such readily distinguishable emotional states as fear and range.[11] Fever[12] and also exposure to cold[13] are known to induce most of the changes—certainly a faster heart rate, vasoconstriction, increased blood sugar, discharge of adrenin and erection of the hairs. Asphyxia at the stimulating stage evokes all the changes enumerated above, with the possible exception of sweating. A too great reduction of blood sugar by insulin provokes the "hypoglycemic reaction"—characterized by pallor, rapid heart, dilated pupils, discharge of adrenin, increase of blood sugar and profuse sweating.[14]

In this group of conditions which bring about in the viscera changes which are typical of sympathetic discharge are such intense and distinct emotions as fear and rage, such relatively mild affective states as those attending chilliness, hypoglycemia and difficult respiration, and such a markedly different experience as that attending the onset of fever. As pointed out earlier by Cannon[15] the responses in the viscera seem too uniform to offer a satisfactory means of distinguishing emotions which are very different in subjective quality. Furthermore, if the emotions were due to afferent impulses from the viscera, we should expect not only that fear and rage would feel alike but that chilliness, hypoglycemia, asphyxia, and fever should feel like them. Such is not the case.

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[11]Cannon, *op. cit.*, 277.

[12]Cannon and J. R. Pereira, Increase of adrenal secretion in fever, *Proc. Nat. Acad. Sci.*, 10, 1924, 247.

[13]Cannon, A. Querido, S. W. Britton and E. M. Bright, The rôle of adrenal secretion in the chemical control of body temperature, *Amer. J. Physiol.*, 79, 1927, 466.

[14]Cannon, M. A. McIver and S. W. Bliss, A sympathetic and adrenal mechanism for mobilizing sugar in hypoglycemia, *Amer. J. Physiol.*, 69, 1924, 46.

[15]Cannon, *op. cit.*, 280.

In commenting on this criticism of the James-Lange theory Angell[16] admits that there may be a considerable matrix of substantially identical visceral excitement for some emotions, but urges that the differential features may be found in the extra-visceral disturbances, particularly in the differences of tone in skeletal muscles. Perry[17] likewise falls back on the conformation of the proprioceptive patterns, on the "motor set" of the expression, to provide the distinctive elements of the various affective states. The possible contribution of skeletal muscles to the genesis of the felt emotion will be considered later. At present the fact may be emphasized that Lange derived no part of the emotional psychosis from that source; and James attributed to it a minor rôle—the chief part of the felt emotion depended on the visceral and organic part of the expression.

(*3*) *The viscera are relatively insensitive structures.* There is a common belief that the more deeply the body is penetrated the more sensitive does it become. Such is not the fact. Whereas in a spinal nerve trunk the sensory nerve fibers are probably always more numerous than the motor, in the nerves distributed to the viscera the afferent (sensory) fibers may be only one-tenth as numerous as the efferent.[18] We are unaware of the contractions and relaxations of the stomach and intestines during digestion, of the rubbing of the stomach against the diaphragm, of the squeezing motions of the spleen, of the processes in the liver—only after long search have we learned what is occurring in these organs. Surgeons have found that the alimentary tract can be cut, torn, crushed or burned in operations on the unanesthetized human subject without evoking any feeling of discomfort. We can feel the thumping of the heart because it presses against the chest wall, we can also feel the throbbing of blood vessels because they pass through tissues well supplied with sensory nerves, and we may have abdominal pains but apparently because there are pulls on the parietal peritoneum.[19] Normally the visceral processes are extraordinarily undemonstrative. And even when the most marked changes are induced in them, as when adrenalin acts, the results, as we shall see, are sensations mainly attributable to effects on the cardiovascular system.

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[16]Angell, *op. cit.*, 260.

[17]Perry, *op. cit.*, 300.

[18]J. N. Langley and H. K. Anderson, The constituents of the hypogastric nerves, *J. Physiol.*, 17, 1894, 185.

[19]K. G. Lennander *et al*, Abdominal pains, especially in ileus, *J. Amer. Med. Assoc.*, 49, 1907, 836 (see also p. 1015).

(4) *Visceral changes are too slow to be a source of emotional feeling.* The viscera are composed of smooth muscle and glands—except the heart, which is modified striate muscle. The motions of the body with which we are familiar result from quick-acting striate muscle, having a true latent period of less than 0.001 sec. Notions of the speed of bodily processes acquired by observing the action of skeletal muscle we should not apply to other structures. Smooth muscle and glands respond with relative sluggishness. Although Stewart[20] found that the latent period of smooth muscle of the cat was about 0.25 sec., Sertoli[21] observed that it lasted for 0.85 sec. in the dog and 0.8 sec. in the horse. Langley[22] reported a latent period of 2 to 4 secs. on stimulating the *chorda tympani* nerve supply to the submaxillary salivary gland; and Pawlow[23] a latent period of about 6 *minutes* on stimulating the vagus, the secretory nerve of the gastric glands. Again, Wells and Forbes[24] noted that the latent period of the psychogalvanic reflex (in man), which appears to be a glandular phenomenon, was about 3 secs.

In contrast to these long delays before peripheral action in visceral structures barely starts are the observations of Wells;[25] he found that the latent period of affective reactions to pictures of men and women ended not uncommonly within 0.8 sec. More recent studies with odors as stimuli have yielded a similar figure (personal communication). According to the James-Lange theory, however, these affective reactions result from reverberations from the viscera. But how is that possible? To the long latent periods of smooth muscles and glands, cited above, there must be added the time required for the nerve impulses to pass from the brain to the periphery and thence back to the brain again. It is clear that the organic changes could not occur soon enough to be the occasion for the appearance of affective states, certainly not the affective states studied by Wells.

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[20]C. C. Stewart, Mammalian smooth muscle—The cat's bladder, *Amer. J. Physiol.*, 4, 1900, 192.

[21]E. Sertoli, Contribution à la physiologie générale des muscles lisses, *Arch. ital. de biol.*, 3, 1883, 86.

[22]J. N. Langley, On the physiology of the salivary secretion, *J. Physiol.*, 10, 1889, 300.

[23]J. P. Pawlow and E. O. Schumowa-Simanowskaja, Die Innervation der Magendrüsen beim Hunde, *Arch. f. Physiol.*, 1895, 66.

[24]F. L. Wells and A. Forbes, On certain electrical processes in the human body and their relations to emotional reactions, *Arch. Psychol.*, 2, 1911, No. 16, p. 8.

[25]Wells, Reactions to visual stimuli in affective settings, *J. Exper. Psychol.*, 8, 1925, 64.

(5) *Artificial induction of the visceral changes typical of strong emotions does not produce them.* That adrenin, or the commercial extract of the adrenal glands, "adrenalin," acts in the body so as to mimic the action of sympathetic nerve impulses has already been mentioned. When injected directly into the blood stream or under the skin it induces dilatation of the bronchioles, constriction of blood vessels, liberation of sugar from the liver, stoppage of gastro-intestinal functions, and other changes such as are characteristic of intense emotions. If the emotions are the consequence of the visceral changes we should reasonably expect them, in accordance with the postulates of the James-Lange theory, to follow these changes in all cases. Incidental observations on students who received injections of adrenalin sufficiently large to produce general bodily effects have brought out the fact that no specific emotion was experienced by them—a few who had been in athletic competitions testified to feeling "on edge," "keyed up," just as before a race.[26] In a careful study of the effects of adrenalin on a large number of normal and abnormal persons Marañon[27] has reported that the subjective experiences included sensations of precardial or epigastric palpitation, of diffuse arterial throbbing, of oppression in the chest and tightness in the throat, of trembling, of chilliness, of dryness of the mouth, of nervousness, malaise and weakness. Associated with these sensations there was *in certain cases* an indefinite affective state coldly appreciated, and without real emotion. The subjects remarked, "I feel as if afraid," "as if awaiting a great joy," "as if moved," "as if I were going to weep without knowing why," "as if I had a great fright yet am calm," "as if they are about to do something to me." In other words, as Marañon remarks, a clear distinction is drawn "between the perception of the peripheral phenomena of vegetative emotion (*i.e.* the bodily changes) and the psychical emotion proper, which does not exist and which permits the subjects to report on the vegetative syndrome with serenity, without true feeling." In a smaller number of the affected cases a real emotion developed, usually that of sorrow, with tears, sobs and sighings. This occurs, however, "only when the emotional predisposition of the patient is very marked," notably in hyperthyroid cases. In some in-

[26]F. W. Peabody, C. C. Sturgis, E. M. Tompkins and J. T. Wearn, Epinephrin hypersensitiveness and its relation to hyperthyroidism, *Amer. J. Med. Sci.*, 161, 1921, 508, (also personal communication from J. T. Wearn).

[27]G. Marañon, Contribution à l'étude de l'action émotive de l'adrenaline, *Rev. franç. d'endocrinol.*, 2, 1924, 301.

stances Marañon found that this state supervened only when the adrenalin was injected after a talk with the patients concerning their sick children or their dead parents. In short, only when an emotional mood already exists does adrenalin have a supporting effect.

From the evidence adduced by Marañon we may conclude that adrenalin induces in human beings typical bodily changes which are reported as sensations, that in some cases these sensations are reminiscent of previous emotional experiences but do not renew or revive those experiences, that in exceptional cases of preparatory emotional sensitization the bodily changes may tip the scales towards a true affective disturbance. These last cases are exceptional, however, and are not the usual phenomena as James and Lange supposed. In normal conditions the bodily changes, though well marked, do not provoke emotion.

The numerous events occurring in the viscera in consequence of great excitement, as detailed by Cannon,[28] have been interpreted as supporting the James-Lange theory.[29] From the evidence presented under the five headings above it should be clear that that interpretation is unwarranted. Since visceral processes are fortunately not a considerable source of sensation, since even extreme disturbances in them yield no noteworthy emotional experience, we can further understand now why these disturbances cannot serve as a means for discriminating between such pronounced emotions as fear and rage, why chilliness, asphyxia, hyperglycemia and fever, though attended by these disturbances, are not attended by emotion, and also why total exclusion of visceral factors from emotional expression makes no difference in emotional behavior. It is because the returns from the thoracic and abdominal "sounding-board," to use James' word, are very faint indeed, that they play such a minor rôle in the affective complex. The processes going on in the thoracic and abdominal organs are truly remarkable and various; their value to the organism, however, is not to add richness and flavor to experience, but rather to adapt the internal economy so that in spite of shifts of outer circumstance the even tenor of the inner life will not be profoundly disturbed.

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[28]Cannon, *op. cit.*, 184.

[29]G. Humphrey, *The Story of Man's Mind*, 1923, 211.

## A Consideration of the Postural Factors

In his discussion of the cerebral processes accompanying emotion, James[30] argued that either there were special centers for them or they occurred in the ordinary motor and sensory centers of the cortex. And if in the ordinary centers, according to his postulate, the processes would resemble the ordinary processes attending sensation. Only that and full representation of each part of the body in the cortex would be needed to provide a scheme capable of representing the *modus operandi* of the emotions. Object—sense organ—cortical excitation—perception—reflexes to muscle, skin and viscus—disturbances in them—cortical excitation by these disturbances—perceptions of them added to the original perceptions; such are the occurrences which result in the "object-emotionally-felt." The strict alternative, however, of cortical processes *or* special centers we need not accept. There may be cortical processes *and* special centers. Whether such is the arrangement we may now consider.

(*1*) *Emotional expression results from action of subcortical centers.* In a paper published in 1887 Bechterev[31] argued that emotional expression must be independent of the cortex because at times the expression cannot be inhibited (*e.g.* laughing from tickle, grinding the teeth and crying from pain), because visceral changes occur which are beyond control, and because it is seen just after birth before cortical management is important. Furthermore, he reported that after removing the cerebral hemispheres from various kinds of animals appropriate stimulations would evoke corresponding responses of an affective character. Noxious stimuli would cause the hemisphereless cats to snarl, the dogs to whine, to show their teeth and to bark; gentle stimuli (stroking the back) would cause the cats to purr and the dogs to wag their tails. Since these effects disappeared when the optic thalamus was removed, he drew the conclusion that it plays a predominant rôle in emotional expression.

In 1904 Woodworth and Sherrington[32] proved that many of the physiological phenomena of great excitement would appear in cats from which the thalamus had been wholly removed by section of the brain stem at the mesencephalon. Strong stimulation of an afferent

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[30] James, *op. cit.*, 123.

[31] W. Bechterev, Die Bedeutung der Sehhügel auf Grund von experimentellen und pathologischen Daten, *Virchow's Archiv*, 110, 1887, 102, 322.

[32] R. S. Woodworth and C. S. Sherrington, A pseudaffective reflex and its spinal path, *J. Physiol.*, 31, 1904, 234.

nerve was required to evoke the "pseudaffective" responses. Although these observations tended to lessen the importance of the thalamus as a center, recent experiments have again emphasized its dominance. In 1925 Cannon and Britton[33] described a pseudaffective preparation—a cat decorticated under ether anesthesia—which on recovery displayed spontaneously the complete picture of intense fury. Further study by Bard (work still unpublished) showed that this sham rage continued after ablation of all the brain anterior to the diencephalon. Only when the lower posterior portion of the thalamic region was removed did the extraordinary activities of the preparation subside. These results clearly point to the thalamus as a region from which, in the absence of cortical government, impulses are discharged which evoke an extreme degree of "emotional" activity, both muscular and visceral.

The evidence just cited is confirmed by observations on human beings. As has been pointed out elsewhere[34] when the cortical processes are abolished by anesthesia, emotional display may be most remarkable. During the early (excitement) stage of ether anesthesia, for example, there may be sobbing as in grief, or laughter as in joy, or lively and energetic aggressive actions as in rage. The surgeon may open the chest or perform other operations of equal gravity, while the patient is pushing, pulling, shouting and muttering; a few minutes later the conscious patient will testify that he has been wholly unaware of what has happened. It is when "laughing gas" has set aside the cortical functions that the subjects laugh and weep. Similar release of the mechanisms for emotional expression is indicated in the depression of cortical activity during acute alcoholism. In all these conditions the drug acts first as a depressant on the highly sensitive cells of the cortex, and thus lessens or temporarily destroys their control of lower centers; only when the drug becomes more concentrated does it depress also the lower centers; but before that stage is reached the lower centers, released from the cortical dominance as in the surgically decorticated animals, show forth their functions in free play.

Consistent with the experimental and pharmacological evidence is the evidence derived from pathological cases. In certain forms of

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[33]Cannon and S. W. Britton, Pseudaffective medulliadrenal secretion, *Amer. J. Physiol.*, 72, 1925, 283.

[34]Cannon, Neural basis for emotion expression, *Wittenberg Symposium on Feelings and Emotions*, 1927.

hemiplegia the patients may be incapable of moving the face on the paralyzed side; if suddenly they are affected by a sorrowful or joyous emotion, however, the muscles, unresponsive to voluntary control, spring into action and give both sides of the face the expression of sadness or gaiety.[35] These cases occur when the motor tract is interrupted subcortically and the optic thalamus is left intact. The opposite of this condition is seen in unilateral injury of the thalamus. A patient described by Kirilzev[36] moved symmetrically both sides of his face at will, but when he laughed in fun or made a grimace in pain the right side remained motionless; at autopsy a tumor was found in the center of the left optic thalamus. This localization of the central neural apparatus for the expressions of pleasure and pain has interesting relations to emotive phenomena commonly seen in so-called "pseudo-bulbar palsy." In such cases there is usually a bilateral facial paralysis, with one side slightly more involved than the other. Voluntary pursing of the lips as in whistling, or wrinkling of the forehead, or making a grimace may be impossible. The intractable facial muscles, however, function normally in laughing or crying, scowling or frowning. These well-executed expressions come in fits and are uncontrollable and prolonged. One patient is described who started laughing at 10:00 o'clock in the morning and continued with few pauses until 2:00 in the afternoon! Tilney and Morrison,[37] who have reported on 173 recorded cases of the disease, found such fits of crying and laughing in seventeen percent of the cases, crying alone in sixteen percent, and laughing alone in fifteen percent. The fits appear as a rule without any of the usual provocations and most frequently are inopportune. The patient may have all the appearances of being convulsed with laughter, yet may not experience any of the feeling which the motions of face and body indicate. Such cases are attributed by Brissaud[38] to lesions of a special part of the cortico-thalamic tract which free a portion of the thalamus from the cortical check. It seems probable, as later evidence will suggest, that afferent thalamo-cortical tracts are also defective. Finally, cases of "narcolepsy" are known in which emotional expression is nearly nil; gibes and insults which enrage or infuriate the normal

[35]G. Roussy, *La couche optique*, 1907, 31.

[36]S. Kirilzev, Cases of affections of the optic thalamus (Russian). Reviewed in *Neurologisches Centralblatt*, 10, 1891, 310.

[37]F. Tilney and J. F. Morrison, Pseudo-bulbar palsy clinically and pathologically considered, *J. Ment. and Nerv. Diseases*, 39, 1912, 505.

[38]E. Brissaud, *Leçons cliniques*, 1894.

person are usually quite without effect. In some of these cases, examined *post-mortem*, were found tumors on the under side of the diencephalon, often affecting the whole hypothalamus.

All these observations, experimental and clinical, consistently point to the optic thalamus as a region in which resides the neural organization for the different emotional expressions. The section in James' discussion, headed "No Special Brain Centres for Emotion" must be modified in the light of this accumulated information. The cortex at one end of the nerve paths as a reflex surface and the peripheral organs at the other end as a source of return impulses make too simple an arrangement. Between the cortex and the periphery lies the diencephalon, an integrating organ on the emotive level, a receiving and discharging station, that on proper stimulation is capable of establishing in stereotyped forms the facies and bodily postures typical of the various affective states. That all afferent paths leading towards the cortex have relays in the diencephalon is a fact of great significance in explaining the nature of emotions.

(*2*) *Thalamic processes are a source of affective experience.* The relaying of all sensory neurones in some part of the optic thalamus has been stressed by Head[39] in his important clinical studies. He and Holmes[40] attributed to this region a sort of consciousness, an "awareness." The effect of anesthesia in abolishing consciousness while leaving emotional expression (thalamic in origin) undisturbed would seem to contradict this view. But even if consciousness is associated only with events in cortical neurones, the important part played by thalamic processes is little disturbed thereby. The relays of sensory channels in the thalamus and the evidence that disturbances in that region are the occasion for intensely affective sensations are all that we need for understanding its relation to the nature of emotions.

Head[41] has cited numerous cases of unilateral lesions in the thalamic region in which there is a marked tendency to react excessively to affective stimuli; pin pricks, painful pressure, excessive heat or cold, all produce more distress on the damaged than on the normal side of the body. Agreeable stimuli also are felt keenly on the

[39]H. Head, Release of function in the nervous system, *Proc. Roy. Soc.*, 92B, 1921, 184.

[40]Head and G. Holmes, Sensory disturbances from cerebral lesions, *Brain*, 34, 1911, 109.

[41]Head, *Studies in Neurology*, 1920, II, 620.

damaged side; warmth stimuli may evoke intense pleasure, attended by signs of enjoyment on the face and exclamations of delight. Again, affective stimuli, such as the playing of music and the singing of hymns, may arouse such increased emotional feeling on the damaged side that they may be intolerable. Affective conscious states have an influence on the damaged side similar to stimuli from the surface receptors. This extravagant influence of affective stimuli, whether from above or below, Head attributed to release of the thalamus from cortical inhibition. It is not an irritative effect, he argued, because it persists for long periods, well after all the disturbances due to the injury have subsided. And since the affective states are increased when the thalamus is freed from cortical control, Head's conclusion is that the essential thalamic center is mainly occupied with the affective side of sensation.

We are now in a position to consider the evidence that the positions and tensions of skeletal muscle make the differentia of emotion. It will be recalled that, although James belittled this element in his theory, his supporters have stressed it, especially since the visceral element proved inadequate (see p. 110). The thalamic cases provide a means of testing the contribution from skeletal muscles, for the feeling-tone of a sensation is a product of thalamic activity, and the fact that a sensation is devoid of feeling-tone shows that the impulses which underlie its production make no thalamic appeal.

Head found that his patients reported marked differences in the feeling-tone of different sensations. A tuning fork may have no effect, whereas patriotic music is felt intensely on the damaged side. All thermal stimuli make a double appeal, to the cortex and to the thalamus. Unselected tactile stimuli act similarly. On the other hand, *sensations which underlie the appreciation of posture are entirely lacking in feeling-tone.* Precisely those afferent impulses from muscles and joints which James and his supporters have relied upon to provide the extra-visceral part of the felt-emotion are the impulses which lack the necessary quality to serve the purpose! The quality of emotions is to be found, therefore, neither in returns from the viscera nor in returns from the innervated muscles.

### A Theory of Emotion Based on Thalamic Processes

The foregoing discussion has disclosed the fact that the neural arrangements for emotional expression reside in subcortical centers, and that these centers are ready for instant and vigorous discharge

when they are released from cortical restraint and are properly stimulated. Furthermore, the evidence is clear that when these centers are released the processes aroused in them become a source of vivid affective experience. That this experience is felt on only one side in hemiplegic cases is a peculiarly happy circumstance, for in the same individual the influence of the same affective stimulus can be observed under normal conditions and compared with its influence when given free rein.

The neural organization for an emotion which is suggested by the foregoing observations is as follows. An external situation stimulates receptors and the consequent excitation starts impulses towards the cortex. Arrival of the impulses in the cortex is associated with conditioned processes which determine the direction of the response. Either because the response is initiated in a certain mode or figure and the cortical neurones therefore stimulate the thalamic processes, or because on their centripetal course the impulses from the receptors excite thalamic processes, they are roused and ready for discharge. That the thalamic neurones act in a special combination in a given emotional expression is proved by the reaction patterns typical of the several affective states. These neurones do not require detailed innervation from above in order to be driven into action. Being *released* for action is a primary condition for their service to the body —they then discharge precipitately and intensely. Within and near the thalamus the neurones concerned in an emotional expression lie close to the relay in the sensory path from periphery to cortex. We may assume that when these neurones discharge in a particular combination, they not only innervate muscles and viscera but also excite afferent paths to the cortex by direct connection or by irradiation. The theory which naturally presents itself is that *the peculiar quality of the emotion is added to simple sensation when the thalamic processes are roused.*

The theory just suggested appears to fit all the known facts. Its service in explaining these facts may be briefly summarized.

When the thalamic discharge occurs, the bodily changes occur almost simultaneously with the emotional experience. This coincidence of disturbances in muscles and viscera with thrills, excitements or depressions was naturally misleading, for, with the rôle of the thalamus omitted from consideration, the obvious inference was that the peculiar quality of the emotion arose from the peripheral changes. Indeed, that inference is the heart of the James-Lange

theory. The evidence presented in the foregoing pages shows that the inference is ill-founded; the sensations from the peripheral changes, contrary to James' view, are "pale, colorless and destitute of emotional warmth," whereas the thalamic disturbances contribute glow and color to otherwise simply cognitive states. The theory now proposed explains how James and Lange could reasonably make the suggestion which they made. The lack of factual support for their suggestion requires another account of emotional origins. This is provided by the evidence that thalamic processes can add to sensation an aura of feeling.

One of the strongest arguments advanced for the James-Lange theory is that the assumption of an attitude does in fact help to establish the emotional state which the attitude expresses. "Sit all day in a moping posture, sigh, and reply to everything with a dismal voice, and your melancholy lingers." On the contrary, "smooth the brow, brighten the eye, contract the dorsal rather than the ventral aspect of the frame, and speak in a major key, pass the genial compliment, and your heart must be frigid indeed if you do not gradually thaw!" Persons who have tried this advice have testified to its soundness, and have been convinced, therefore, of the truth of the claim that the moods have followed the assumed attitudes. Not all agree, however, that mimicking the outward appearance of an emotion results in the emotion itself. James suggested that the explanation of the discrepancy lay in variations of involvement of the viscera in the artificial expression. As shown above, however, the visceral changes offer only unreliable support for that idea. Again the processes in the thalamus offer a reasonable and simple explanation. As the cases reported by Head have shown, emotions originating from memories and imagination affect more intensely the half-thalamus that has been released from motor control than they affect the normal half. This shows that cortical processes may start thalamic processes and thus arouse an affective return from that portion of the brain. And if in addition a typical emotional attitude is assumed the cortical inhibition of the thalamic neurones with reference to that attitude is abolished so that they have complete release. Under such circumstances the enacted emotion would have reality. On the other hand a purely cortical mimicry of emotional expression without thalamic involvement would be as cold and unaffective as some actors have declared it to be. Whether the emotion

results or not, the thalamic theory of the source of feeling offers a more satisfactory explanation of the effects of assumed postures than does the James-Lange theory.

The cases of release of the thalamus from cortical control on one side, with accompanying ipsilateral intensification of emotional tone, present an insurmountable obstacle to the James-Lange theory. Neither the thoracic nor the abdominal viscera can function by halves, the vasomotor center is a unity, and the patients certainly do not engage in right- or left-sided laughter and weeping. The impulses sent back from the disturbed peripheral organs, therefore, must be bilaterally equal. For explanation of the unsymmetrical feeling we are driven to the organ which is functioning unsymmetrically—*i.e.* the thalamus. It is there that the suggested theory places the source of the emotions.

Another serious difficulty for the James-Lange theory is the evidence that the emotion increases in intensity although the expression is checked. Indeed, there are psychologists who maintain that the emotional state lasts only so long as there is inner conflict between the impulse to act and the hesitant or prudential check on that impulse. So long as the check prevails, however, the organic changes supposed to be the source of the feeling are suppressed. How then can there be felt-emotion? Two answers to this question may be found in James' argument. First he denies the objection. "Refuse to express a passion," he wrote, "and it dies." "Count ten before venting your anger, and its occasion seems ridiculous." On the other hand, he appears to admit that a pent emotion may operate disastrously. "If tears or anger are simply suppressed, whilst the object of grief or rage remains unchanged before the mind, the current which would have invaded the normal channels turns into others, for it must find some outlet of escape. It may then work different and worse effects later on. Thus vengeful brooding may replace a burst of indignation; a dry heat may consume the frame of one who fain would weep, or he may, as Dante says, turn to stone within." There is no intimation that vengeful brooding, being consumed by a dry heat, and turning to stone within are not emotional experiences. Instead of recognizing them as such, however, James stressed the importance of training for repression of emotional display. These rather equivocal and indecisive comments leave untouched the common testimony that intense fear, for example, may be felt, with a pathetic sense of helplessness, before any overt act occurs, and that

scarcely does the appropriate behavior start than the inner tumult begins to subside and the bodily forces are directed vigorously and effectively to serviceable ends. The difficulties of the James-Lange theory in meeting this situation are obvious. If there is a double control of behavior, however, both the inner conflict with its keen emotional accompaniment and the later partial subsidence of feeling are readily explicable. The thalamic patterned processes are inherent in the nervous organization, they are like reflexes in being instantly ready to seize control of the motor responses, and when they do so they operate with great power. They can be controlled, however, by the processes in the cerebral cortex, by processes conditioned by all sorts of previous impressions. The cortex also can control all the peripheral machinery except the viscera. The inhibited processes in the thalamus cannot set the organism in action, except the parts not under voluntary control, but the turmoil there can produce emotions in the usual manner, and possibly with greater violence because of the inhibition. And when the cortical check is released, suddenly the conflict is resolved. The two controls formerly in opposition, are now coöperative. The thalamic neurones, so long as they continue energetically active, provide the condition for the emotion to persist, as James claimed it does, *during* the manifestation. The new theory, therefore, not only avoids the difficulty of the James-Lange theory, but accounts satisfactorily for the poignancy of feeling in the period of paralyzed inaction.

In relation to the double control of the response there is another point that may be emphasized. McDougall[42] has objected to the James-Lange theory on the ground that it is admittedly concerned with the *sensory* aspect of emotion; it pays little or no attention to the always present and sometimes overwhelming *impulsive* aspect of the experience. The localization of the reaction patterns for emotional expression in the thalamus—in a region which, like the spinal cord, works directly by simple automatisms unless held in check—not only accounts for the sensory side, the "felt emotion," but also for the impulsive side, the tendency of the thalamic neurones to discharge. These powerful impulses originating in a region of the brain not associated with cognitive consciousness and arousing therefore in an *obscure* and *unrelated* manner the strong feelings of emotional excite-

[42]W. McDougall, *Outline of Psychology*, 1923, 328.

ment, explain the sense of being seized, possessed, of being controlled by an outside force and made to act without weighing of the consequences.

Finally, the view that thalamic processes add feeling-tone to sensation meets satisfactorily a difficulty which the James-Lange theory encountered in explaining the "subtler emotions." James had to assume indefinite and hypothetical bodily reverberations in order to account for mild feelings of pleasure and satisfaction. If a warm test tube, however, is capable of yielding keen delight on the damaged side in a case of thalamic injury, it is clear that almost any object or situation which can rouse thalamic processes can add affective quality to sensation. And just as a stimulus can become conditioned for a certain motor or glandular response, so likewise a stimulus can be conditioned for the patterns of neurone action in the thalamus. When that stimulus recurs the emotion recurs because the pattern is activated. In such manner we may consider that richness and variety of our emotional life are elaborated.

# ANGER AND FEAR: THEIR PROBABLE RELATION TO EACH OTHER, TO INTELLECTUAL WORK, AND TO PRIMOGENITURE

By GEORGE M. STRATTON, University of California

The present paper would continue the account of a study of some of the less-known conditions which influence our emotions and are influenced by our emotions, in the hope of adding somewhat to our knowledge of the causal connections of these important processes. While there has been a wider gathering of evidence upon that particular aspect of the problem which was considered in a paper preceding this, I shall here enter into questions not touched in that preliminary report.[1]

## I. METHOD

The method of gathering the evidence, which was barely outlined in that brief statement, should now be fully described.

At the infirmary of the University of California a medical examination is made of each student at the time of his entrance, whether as a freshman or with advanced standing. He is asked by the examining physician—who, for the men, is a man, and for the women, a woman— which of certain diseases the student may have had, and his age when each occurred. To the record of these examinations I have had access, through the kindness of Dr. Legge and Dr. Cunningham, the university physicians; and this is the source of our knowledge of the personal history, with respect to disease, of every student whose emotions were under observation. The diseases included in this study are: appendicitis, asthma, Bright's disease, constipation, diphtheria, encephalitis, headache, heart trouble, infantile paralysis, infections (inflammation of the bowels, discharging ear, etc.), influenza, malaria, meningitis, neurasthenia and nervousness, pleurisy, pneumonia, rheumatism, scarlet fever, smallpox, St. Vitus' dance, tonsilitis, tuberculosis, and typhoid fever. The expressions "with a

[1]G. M. Stratton, Emotion and the incidence of disease, *J. Abn. Psychol. & Soc. Psychol.*, 21, 1926, 19-23.

history of disease" and "without a history of disease" will be understood, then, to refer in brief to these particular twenty-three diseases, and to the time up to the examination just described. For, unfortunately, there was not available a record of these or other diseases that may have occurred after entrance to the University, because no later examination was compulsory, and any illness after entrance might be treated either at the Infirmary or at home or otherwise, according to choice; so that the Infirmary records are not sufficiently full for a knowledge of this later time.

Intelligence examinations of these same students were made by means of the Alpha test used in the Army. The scholarship of every student was determined by computation from the record of his work in all the studies of his first two years at the University of California, the Recorder of the University, Mr. Sutton, generously permitting us access to his files. Measurements of the head and face, together with a judgment of the color of hair and eyes, were made by the writer and a trained assistant, Mr. Blodgett, the students coming to a place suitable for examining them singly. There was also obtained from each of them a written statement as to his parentage and the number of his brothers and sisters, and as to the order of his own birth, whether he was the first-born, or the second-born, and so on.

With regard to their emotions the procedure was as follows. Those taking the course in general psychology, all of them sophomores or higher, were, in three different years, asked to coöperate, outside of class, in a study of emotion. In all of the three classes it was made clear that the assistance would be of value only if done with accuracy and good will. In two of the classes there was assurance that the readiness or failure to make returns would in no way affect a student's grade in the course; in one of the classes nothing was said as to this. I believe that in general we had the honest coöperation of those who made their reports. I am indebted to Dr. Brown, in charge of one of the classes, and to Dr. Miles of Stanford, temporarily with us in another year, for their care that the best results possible under the circumstances should come from their classes. Nearly all of the extended statistical computations needed for this work were made by Mr. Robert Tryon himself or under his charge.

Typed instruction, added to an oral explanation, were given to the students on a Friday afternoon, with the request for written returns at the next meeting of the class on the Monday following. Three full days were thus allowed for observation, and always for

only one of the two emotions—let us say anger. About six weeks later the reports were upon the other of the two, the order of them being changed with different classes.

The instructions regarding anger were essentially as follows:

(page 1)

UNIVERSITY OF CALIFORNIA

Department of Psychology

During the time between the present and the next following session of this class, you are earnestly requested to keep an accurate record of your behavior in every one of the situations described on pages 2 and 3. Your report will be of value both for your own training in psychological observation and for an important scientific research now in progress. Your report will be counted strictly confidential. Please do not talk with any one about it (unless with your instructor, to clear up doubts) until your report has been handed in.

The record should be kept in the following way. Write your name on page 5. At once read carefully and repeatedly, until you have them well in mind, the descriptions of the situations on pages 2 and 3. Then, as soon as possible after any one of the situations occurs, note in column *a* on page 5, the day and hour of its occurrence, and in column *b* of the same line on that page, the capital letter indicating which of the situations occurred. Then after a careful study of the six statements which tell the different grades of effect which the situation might have upon you, select the particular grade which comes closest to the actual effect which the situation caused in you; and enter in column *c* the Roman numeral which indicates this actual effect. Finally, in column *d* note the day and hour when your record of this particular situation was made. The value of the record is greatly increased if made soon after the occurrence; it is worthless if six hours or more elapse between the occurrence and your record of its effect.

If any of the situations occurs more than once, make a separate record of its occurrence and its effect each time it occurs, no matter how often this may be. But do not include anything merely remembered from some time before the hour when you receive this request.

Please bring your report to the next general meeting of the class. Be sure that your name is on your report. If none of the situations has occurred, please return the blank with your name upon it, nevertheless, and state that none has occurred. If for any serious reason you wish to be excused from this exercise, you are at liberty to apply to your professor for excuse.

(pages 2, 3, and 4)

SITUATIONS

(A) A friend of yours is unjustly criticized in your hearing.

(B) Some one argues with you insistently against what you believe to be important.

(C) You hear for the first time that a certain person has made a slighting remark about you.

(D) You are rebuked before others by one of your companions.

(E) An acquaintance passes without recognizing you, and you believe yourself to have been intentionally 'cut.'

(F) Some one fails to keep an appointment with you and did not notify you that the appointment could not be kept.

(G) Something of yours which you need is borrowed without your permission.

(H) You are persistently and unpleasantly teased.

(J) There is continued music, conversation, typing, or the like, which disturbs you while you are trying to study.

(K) There is continued music, conversation, typing, or the like, which disturbs you while you are trying to sleep.

(M) At a theatre, concert, or other public place where you are trying to hear what is being presented, a stranger makes remarks or otherwise acts in a way to distract your attention disagreeably.

(N) You are deprived of an anticipated pleasure upon which you had set your heart.

(O) You have to do a disagreeable task because some one has shirked his duty.

(P) You are treated discourteously by an employee in some office, shop, street car, train, or the like.

(Q) A stranger jostles or crowds you without apology.

(R) 'Central' of the telephone service delays for some time to give attention to your call, or does not seem to be trying to get the connection you wish, or connects you with the wrong number.

(S) While telephoning, you are cut off in the midst of your conversation.

(T) You hurry for a street car, and miss it by a very narrow margin.

(V) You hurry for a ferry boat or for a connecting train and miss it by a very narrow margin.

### Possible Effects of the Given Situations upon You

(I) You are not annoyed or irritated in even the slightest degree.

(II) You are slightly annoyed, but there are no physical symptoms of emotion, so far as you can notice.

(III) You are irritated, you feel moderate displeasure; and there are physical symptoms of emotion (a very slight frown, or flush, or muscular tension, perhaps) but these are so slight that (it seems to you) they could be noticed only by an attentive observer.

(IV) You feel an impulse to make cutting remarks; and the physical symptoms of your anger (such as frown, or flushed face, or irregular breathing) must (so it seems to you) be evident to any observer. But the symptoms do not have the character described in the following paragraph.

(V) Your mental equilibrium is upset so that your work is interfered with; and you feel an impulse to attack the offender physically, or to throw things, or stamp with your foot, or slam the door, etc.

(VI) You are stirred to passionate, violent anger; your physical as well as your mental equilibrium is upset; and one or more of the following physical symptoms are present: trembling, nausea, loss of appetite, inability to talk coherently (voice shaking, sputtering, choking sensation, or the like) pallor, weeping.

(page 5)

Name..... Date.....

| a | b | c | d |
|---|---|---|---|
| Day and hour of the occurrence (*e.g.* 16th, 5:20 p. m.) | Kind of situation (by capital letter only) | Effect of the situation (by Roman numeral only) | Day and hour of recording (*e.g.* 16th, 5:25 p. m.) |
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Continue the record on the back of this sheet if necessary.

If, for some cause wholly outside the situations described in the instructions, your physical or mental condition was quite upset during the time covered by any or all of the observations, please state this fact here.

The instructions for fear were the same as these, except of course in the description of situations and of the possible grades of effect of these situations, which were as follows.

SITUATIONS

(A) You are driving an automobile and you narrowly escape a serious collision.

(B) You are driving an automobile and you narrowly escape hitting a pedestrian.

(C) You are in an automobile but are not driving it, when you narrowly escape a serious collision.

(D) You are in an automobile, but are not driving it, when you narrowly escape hitting a pedestrian.

(E) You are afoot in a street where there is heavy traffic; and you have to stand still between trolley cars or automobiles passing close to you and on both sides of you at the same time.

(F) Afoot, you are crossing the street and narrowly escape being run down by a swift vehicle (trolley car, truck, electric train, automobile, etc.).

(G) While you are standing within about six feet of the track of a steam railway, an engine and train pass you at a high speed.

(H) You find yourself looking down from a high place from which the drop is precipitous.

(J) There is an alarm of fire in or next to the very house in which you are at the moment.

(K) You are in or near the house where you are living and you hear an alarm of fire, but it is clearly not in your house or in the house next to it.

(L) You are away from the house where you are living, but in the same town or city, and you hear an alarm of fire.

(M) A large dog comes barking or growling loudly at you.

(N) You are walking or standing in a street and see some one nearly run over by a passing vehicle (automobile, truck, wagon, trolley car, train, etc.).

(O) As you are quietly sitting or standing, a loud, inanimate sound (perhaps of a falling object, or of a steam whistle) occurs suddenly and unexpectedly near by.

(P) In the house, when all is quiet, you and another person suddenly and very unexpectedly come upon each other.

(Q) After dark, but before 10:30, you enter your own unoccupied room which is dark, and which you can light only by going well into the room; you yourself have not been in the room since it grew dark.

(R) After dark and later than 10:30 you enter your own unoccupied room which is dark and which you can light only by going well into the room; you yourself have not been in the room for some hours.

(S) Late at night you enter the house where you are living; others you know are in it, but the house as you enter it is dark.

(T) Late at night you are alone in an unfrequented place which is but dimly lighted and you hear approaching you some one you can not see and whom you suppose to be a stranger.

(U) Late at night you are alone and pass through some unfrequented place where all is still and there are dark shadows.

(V) You are in your room which you occupy alone; and the room is dark; and you are awakened by sounds as though some one were stealthily moving about either in your room or very near it.

(W) You see some one approach, apparently without his knowing it, a high place from which the drop is precipitous.

### Possible Grades of Effect of the Given Situation

(For each situation, each time, choose the grade which is closest to the actual effect in your case.)

(I) You feel no fear or trepidation or timidity, in even the slightest degree.

(II) You are slightly afraid; but you have no physical symptoms of fear, so far as you can judge by your own feelings.

(III) You feel moderate fear; and there are mild physical symptoms of it—perhaps some quickening of your heartbeat, some disturbance of your breathing—but these are all so slight that (it seems to you) they could be noticed only by an attentive observer.

(IV) You feel strong fear; and the physical symptoms of it—such as trembling and the disturbance of your breathing and heartbeat—must (so it seems to you) be evident to any observer. But the symptoms are not so strong as those described in Grade V.

(V) Your fear is so intense that your mental equilibrium is quite upset; and along with your trembling or shivering, and the disturbance of your breathing and heartbeat, you feel a decided impulse, which you can control, however, to cry out or to hide or to take flight.

(VI) You are stirred to a fear so intense that both your physical and your mental equilibrium are upset; and one or more of the following physical symptoms are present: a wild beating of your heart, gasping, very labored breathing, utter weakness or faintness, cold perspiration, headlong concealment or flight.

## II. Defects and Advantages of the Method

This method of gathering the facts of emotion has obvious shortcomings. It gives no precise and instrumental record of physiological reactions during emotion, but only the persons' unaided observations of their own behavior, and usually (and preferably) not during, but immediately after, the emotional response. Unintended and intended deception is not impossible. If there were no advantages to weigh against these defects, the method could hardly be approved.

But the advantages are that it brings us into touch with the behavior of a large number of persons, probably honest and certainly intelligent, and in situations which, even if slight and often commonplace, are those of real life. There is an advantage also in having the observations include somewhat more than the physiological reactions already known to be characteristic of certain emotional responses. We still know little of the deeper conditions which distinguish some of the emotions, knowing more of what they have in common. It may, consequently, seem well to observe anger and fear in their gross characters, in many persons, and as distinguishable reactions. The present method is intended merely to supplement other methods, for use *ad interim*, to give way as we make advances in our instrumental technique and in the interpretation of the records of that technique.

Finally, in the present method each person who tells of himself is asked about a particular occurrence in a very recent time; and the record, made upon the day of the event itself, is simple and according

to a fairly definite scale, which requires no generalization about oneself, but leaves this to the experimenter, to be arrived at by computation.

In such computation a score was attached to every one of the six "possible grades of effect" for every one of the situations above described. This score was based on the frequency of each of these possible grades in the actual array of grades for that particular situation in the returns by the class as a whole.

The formula used, from Kelley,[2] is

$$d = \frac{z_1 - z_2}{q_1 - q_2}$$

where d is the amount, expressed in terms of the standard deviation, by which a given grade differs from the mean grade of a given situation. The mean score for the particular group in each particular situation was arbitrarily expressed as 100.

From this computation it was possible to give to every person a score for everyone of his responses to an anger-situation, there being now substituted for his own grading, based on the rough scale in his sheet of instructions, our own more precise grading. The individual's general score for anger was the average of his scores for his several anger-reactions. Similarly for fear. Finally, his general score, when it came to be combined with the scores of other persons, was given a weight proportioned to the number of responses from which his general score was derived. Preliminary computations were made also wherein all persons were given equal weight; and again wherein their weights were proportional to the number of reactions up to four, and for more than four responses the weights were all equal to that of four. But in the samples tried according to these different systems no significant changes in the relations seemed to appear. I do not believe that any of the conclusions drawn depend on the system of weighting which was finally used.

## III. Trustworthiness of Scores

Now something must be said regarding the trustworthiness of these scores of anger and of fear.

One may reasonably expect that the strength of a person's response to one of the situations described would give but an uncertain index to the strength of his response to a *different* situation. One's

[2] T. L. Kelley, *Statistical Method*, 1924, 101.

irritation at being cut off in the midst of a telephone conversation would give little knowledge of one's irritation at sounds near-by when trying to sleep. But in a measure this is true of the *same* situation; the intensity of one's anger when the stimulus comes the first time will hardly be the same as that when it comes again. There would probably be a difference in the effect if the externals were identical; for the internals, the state of the reagent himself, could hardly be the same. There would be summation and increased effect, or accommodation and decreased effect; and the reagent's attitude would be altered by a host of other variable conditions which go with hunger and satiety, fatigue and rest, and with the general health of the psychophysical system. On one day a slightest word will anger us; on another it will cause a smile of indifference.

There is a further feature to make our situations to be of variable effect. Any one of the descriptions of situations in the printed instructions given to my observers does not define with precision a stimulus which is always the same even externally. "You hear for the first time that a certain person has made a slighting remark about you" is virtually a general idea which can fit a wide range of individual cases. Every time that the situation is repeated, the "certain person" will probably be a different person whose different relation to you will give a different effect to his slighting remark, which also will be different.

It is to be expected, therefore, that with all these variables, inner and outer, the score of an individual, when it is based on one of his anger situations, will be very different from his score based upon some other situation, or even upon the recurrence of the same situation. It would be surprising were there any but the lowest correlation. There is in fact but a low correlation between successive scores of the same person, which increases when the scores are based on more and more of his emotional responses.

Thus for one of my groups of observers, reporting on their reactions at two well separated stretches of time, the correlation between the scores based on the first report and the scores of the same persons based on the second report was as follows (see Table I).

We are dealing, then, with emotional responses any one of which is but a rough index to the emotional behavior of the individual generally. This roughness would be a fatal objection to using any single score based on but a few reactions as a sign of a particular person's true place emotionally among his fellows. But there is no fatal ob-

jection to using these scores cautiously for light upon a group as a whole to determine this group's probable place with reference to another group as a whole. A spirit of caution will show itself particularly in this, that any differences or correlations which appear at all and are sufficiently large in comparison with their probable errors, must be understood, less as giving the true amount of the emotional difference or correlation between group and group than as indicating

TABLE I

| Emotion | No. of emotional reactions in each individual's report | No. of persons reporting | No. of emotional reactions in all | Correlation between their successive scores | Probable error of this coefficient |
|---|---|---|---|---|---|
| Anger | One or more | 144 | 235 | +.145 | .043 |
| | Two or more | 64 | 155 | +.318 | .049 |
| | Three or more | 18 | 63 | +.367 | .074 |
| Fear | One or more | 85 | 156 | +.276 | .050 |
| | Two or more | 38 | 109 | +.327 | .058 |

that a difference or a correlation exists between these groups, and that it takes a known direction. The probability is higher that there is some relation and that it takes the general direction indicated than that it is of the precise quantity stated by our numbers.

Coming now to some of the problems to which these preparations looked, I have borne in mind the importance of considering the men and the women separately as well as together, but the report upon their differences will be postponed; the groups here considered are always of both sexes, with the women outnumbering the men. I shall also postpone all further report upon disease, where there are matters beyond what was given earlier; and also upon racial differences as revealed in connection with head measurements and the color of hair and eyes. There will be discussed only the questions as to the relation of our two emotions to each other, to intelligence and scholarly achievement in college, and to order of birth.

## IV. MUTUAL RELATION OF ANGER AND FEAR

It is not difficult to believe that anger and fear are related to each other, although observation does not easily make clear what the relation is. Very little may be needed in the external situation—often nothing at all, but only a change in the internal situation—to shift

the response from one of fear to one of anger. Sudden fear relieved—as when one sees his child in danger, and then sees that the danger has been escaped—may pass over into anger.

But any case of transition from one to the other emotion, or a clear likeness in their stimuli, or a partial identity in the physiology of these emotions—these leave unanswered a certain question as to disposition. We should still wish to know whether a person who is the more inclined to anger, is or is not probably the more inclined to fear.

The common-sense expectation would perhaps be, to find the two tendencies opposed to each other; to find that a person disposed to react the more angrily would probably be disposed to react the less fearfully.

The present evidence seems to point in a different direction; to make it probable that the persons who in anger-situations are given to anger-reactions more intense than those of the average, are given in fear-situations to react more fearfully than the average individual. Table II shows the direction in which the evidence points.

TABLE II

| No. of persons reporting at least three reactions of anger and three of fear | Correlation between the anger-scores and the fear-scores of these persons | Probable error of this coefficient |
|---|---|---|
| 121 | +.24 | .03 |

Other computations have been made, and in every group there is found some correlation between anger and fear, and the correlation is always positive.

This may mean, in part or whole, that a person tends to exaggerate in a like direction his score in different emotions. It may mean, however, in part or whole, that there is a tendency to a concurrence of the two forms of emotional disposition; that the one tendency will be found more often to accompany than to exclude the other. Anger in a given situation, and for the time being, may or may not exclude fear, but the psychophysical basis for strong anger (according to this interpretation of the evidence) is also in part the basis for strong fear; and weak for weak.

Yet one should hesitate to apply this statement generally. For it may be found true only in certain ranges of mental life. Dr. Morrison did not discover any connection between anger and fear in per-

sons of defective intelligence. What will be reported later in the present paper, along with what was given in the report preliminary to it, leads one to the idea that there is disconnection as well as connection between the tendencies to anger and to fear. They appear to have a different degree of connection with a personal history of disease and with primogeniture. It would seem probable that there is a difference of *Anlage* for the two emotions, as well as a difference in the composition of the two emotional states.

But this need not conceal what may be common to both of them. Both anger and fear are defense-reactions, aroused when some desire of ours is felt to be endangered and when our habitual and calm reactions do not appear adequate to avert the danger. Not only are the same fundamental desires at the back of both of these emotions—the desires for free movement, possessions, reputation, and much else, both for oneself and for others to whom one is attached—but many of the impulses which enter into the one emotion enter into the other also. According to the individual's grasp of a given situation, inner and outer, will he hide or run away or stand his ground defiantly or rush forth to fight. In any event the objective is much the same—to free oneself from the threatened frustration of one's desire. Neither the psycho-galvanic reflex nor the method used by Cannon has thus far revealed what it is that distinguishes anger from fear. The evidence described in the present paper suggests at least, while not demonstrating, that there is a common psychophysical basis for the two emotions, which would help to explain the resemblance in their physiology. But this evidence would also leave wide room for contrast between them, both in their roots and in their fruits.

## V. Anger, Fear, and Intellectual Work

The facts gathered reveal no connection between either anger or fear and intelligence. Our various computations arrive at coefficients of correlation which are approximately zero. Whether this points to a peculiar set of circumstances in the higher levels of intelligence, I am unable to judge. It is by no means improbable, however, that a connection between emotion and intelligence exists at least in lower grades of intelligence. For Dr. Morrison found a fairly high degree of connection, positive, between intelligence and anger, but not between intelligence and fear, in the feeble-minded.[3]

[3]B. M. Morrison, A study of the major emotions in persons of defective intelligence, *Univ. of Calif. Publ.*, 3, 1924, 73-145.

But what of success in college studies? Do the constitution and habits which go with intense anger promise more or do they promise less for intellectual work? Similar questions may be asked regarding fear.

Regarding anger there appears to be some opposition between the intensity of response and the standing in courses generally. By converting the class grades so that a high grade means high standing, there is found a low negative correlation between anger-scores and intellectual-accomplishment scores. A similar relation holds also for fear (see Table III).

TABLE III

| Emotion | No. of persons whose emotion-reactions and academic grades were used | No. of emotional reactions on which the emotion scores were based | Correlation between emotion-scores and academic grades | Probable error of this coefficient |
|---|---|---|---|---|
| Anger | 371 | 1683 | −.102 | .016 |
| Fear | 256 | 635 | −.109 | .026 |

Both of these coefficients, although small, are so related to their probable errors as to indicate a real connection. When we remember that the connection between college grades and any other discernible fact, even that of intelligence, is not high, we shall be the less inclined to regard such a correlation as of no importance because it is small.

If this concomitance should be fully verified, it would mean that success in scholarly work is the more probable the less the individual is inclined to anger or fear. Individuals irascible or timid are, on the whole, apt to accomplish less.

Were we to enquire into the cause of this relation, our evidence would not promise that this cause lies in a lower degree of intelligence in those with a readier anger or fear. The failure to find any correlation between intelligence and either of these emotions would not encourage us to seek the reason there. It is conceivable, and I believe it probable, that a greater readiness to be irritated or fearful is a direct impediment to scholarly achievement. This is probable, I feel, even though we have evidence that, with other things equal, to assume something like an angry attitude toward a problem, as though to down it, increases the chance of solving it.[4] But the as-

[4]Stratton, *Developing Mental Power*, 1922, 33.

sumption of a fighting attitude toward a problem is, of course, different from the assumption of a fighting attitude toward the person who sets the problem, or toward a classmate or a roommate, or the elderly academic fates that do their small best to lessen college pleasures. The person inclined to be irritated is too often wasteful of his energy; and, changing whatever needs to be changed, the same is true of fear.

But to look for our explanation in a somewhat different direction, and away from the energy wasted by an easy slipping into angers and fears, we might bear in mind that those who are the readier to be angry or afraid have probably a smaller store of habitually available energy to start with. Because the energy needed is not immediately at hand, they sooner and oftener call upon their reserves of energy; this is the use and meaning of their more intense emotional responses. Now persons with less of habitually available energy would by this very fact be at a disadvantage in their cognitive activity; there would be less energy in and behind these functions. The heightened emotions of anger and fear, if this be true, would go with poorer scholarship, not because these emotions themselves interfered with application to a task and to its comprehension, but because they were common symptoms of a psychophysical constitution with less readily available vigor.

But the one of these hypotheses does not exclude the other. Both of them may go together in a ratio of importance as yet unknown to us. Both of them are in accord with a view of the function of the sthenic emotions set forth elsewhere.[5]

## VI. Order of Birth, as Related to these Emotions

Passing now to another question, does the order of birth have any bearing upon the emotional disposition? Does the first-born child, when it becomes mature, tend to show, in the average reactions of his kind, some emotional divergence from the child of a later order of birth?

The record of persons who are first-born, when set beside that of persons not first-born, may be presented in summary in Table IV.

There appears to be but a very low probability that a difference in regard to fear really exists between the first-born and those not first-born—a probability represented by the ratio of 3 to 1.

[5]Stratton, An experience during danger, and the wider functions of emotion, *Problems of Personality: Studies in honor of Dr. Morton Prince*, 1925, 47-62.

But in regard to anger the probability of a real difference is considerably more than 99 to 1. The first-born have on the average an anger-score appreciably larger than have those who are not first-born, and the relation which this difference between the two averages bears to the probable error of the difference makes us fairly secure in believing it not to be wholly accidental.

TABLE IV

| Emotion | Order of birth | No. of persons | No. of emotional reactions | Mean emotion-score | Probable error of the mean | Difference between the two means | Probable error of the difference |
|---|---|---|---|---|---|---|---|
| Anger | First | 302 | 825 | 101.72 | .56 | 4.60 | .68 |
| | Not first | 544 | 1473 | 97.12 | .44 | | |
| Fear | First | 273 | 642 | 100.16 | .68 | .92 | .96 |
| | Not first | 441 | 955 | 99.24 | .64 | | |

If we were now to enquire whether the emotional disposition of the first-born here suggested is due to their position among their brothers and sisters—to their being lordlings over these by primogeniture—or is due to something quite independent of this, can anything be found in the present evidence which will help us toward an answer?

When we divide our first-born into two classes, of the first-born who had brothers or sisters, and of the first-born who were 'only' children, the number in the latter group, the 'only' children, is smaller than is needed for secure judgment, and may for this reason not reveal facts which would appear were there a larger number of persons in the group. But so far as our evidence goes, it suggests that the tendency to greater anger in the first-born does not appear to be connected with the presence or absence of younger brothers or sisters in the family. In summary, the figures are shown in Table V.

The difference between the first-born who were 'only' children and the first-born who were not is—both for anger and for fear—insignificant when considered in the light of the probable error of the difference. Such emotional tendencies as are here indicated to exist in the first-born and not in those of later birth would seem to be con-

nected with the primogeniture itself and with the relation which the child bears to the parents. This is not strange. For the first-born stands in a different relation to its parents, both physiologically and socially, from that of children born in a later order. For the first-born the prenatal conditions are different, the conditions of birth are different, the conditions of parental training after birth are different. These factors would suffice to account for the difference. But the

TABLE V

| Emotion | First-born with brothers or sisters | No. of persons reporting their emotional reactions | No. of reactions reported | Mean emotion-score | Probable error of the mean | Difference between the mean scores of the two groups | Probable error of the difference |
|---|---|---|---|---|---|---|---|
| Anger | Yes | 211 | 553 | 101.44 | .68 | .84 | 1.20 |
| | No | 91 | 272 | 102.28 | .88 | | |
| Fear | Yes | 175 | 408 | 100.80 | .88 | 1.76 | 1.32 |
| | No | 98 | 234 | 99.04 | 1.00 | | |

present evidence gives no clue by which we can select with any just confidence the particular factors which are most intimately associated with the difference here revealed, and which would presumably be causally connected with the difference. Were it ultimately to be established that there is an emotional mark characteristic of the first-born as a class, this would bear upon the long-standing dispute concerning the concomitants, both bodily and mental, of primogeniture. The present evidence seems to indicate that the first-born is apt to be more irascible, to his advantage or disadvantage; and that this quality in him is not due to the presence or absence of brothers and sisters, but is independent of this special feature in family training.

# A BASE FOR THE APPROACH TO QUANTITATIVE STUDIES IN THE AESTHETICS OF MUSIC

By Carl E. Seashore, University of Iowa

Experimental psychologists have often avoided aesthetic problems because matters of feeling are intangible and many fields of aesthetics will probably continue to be resistent on this score. It is the purpose of this note to point out that within the field of the psychology of music we may have what I shall call a base or zero line from which all measurements in aesthetics of music may begin. This is the assumption that *beauty in music consists in artistic deviation from the regular, the rigid, or the fixed in each attribute of sound.*

Such an hypothesis is made possible in this field because music has a physical medium which lends itself completely to analysis, measurement, and interpretation; namely, the sound wave. For everything that is conveyed from singer or player to listener as music is conveyed on the sound wave. The sound wave may be intercepted with the camera, recorded, analyzed, and measured with a high order of precision. If this hypothesis is justified it makes all problems in musical aesthetics tangible in that the medium of measurement available furnishes us a zero point or base line within which or away from which all measurements may be expressed. I confess that before I recognized this fact many of our measurements were quite in the air because we did not have any base of reference. With such a base all our measurements become parts of a comparatively simple system of extension from fixed starting points. I know of nothing analogous to this in any other sense within the principal aesthetic fields.[1]

This maxim on which I propose that the aesthetics of music shall be built, gains further significance from the fact that the variables in the sound wave may be reduced to a small number in the elementary forms; namely, frequency, amplitude, duration, and wave-form. These are the four and the only 'plastic media' in terms of which beauty or ugliness may be created in music. In dealing with the more

[1]True, color and form can be measured and one may start with spectral color of a given wave-length, but such measurements of color are not freely available for ordinary use in aesthetics and, where they are available, the problem of interpretation becomes exceedingly complicated.

complicated features of music such as rhythm, harmony, or complex dynamic features these are ultimately referable to the four basic aspects of the sound wave which are measurable.

In this law of deviations I include, of course, the base itself for each factor; such as perfect intonation, rigid time, or pure tone, which in themselves are beautiful for certain purposes but are comparatively rare in actual music. It remains to be shown by experiment whether or not this hypothesis has complete validity; but as many phases of the subject as I can think of at the present time seem to conform to this principle that beauty consists in deviation from a fixed base such as true pitch, rigid time, or rhythmic pattern, uniformity in loudness, or a pure tone. Ugliness, of course, comes through the same media as beauty. In recent years much of our experimentation has been based upon this hypothesis with good results.

Just as we account for the vast variety of tastes in terms of the four fundamental tastes, salt, sour, bitter, and sweet, so musical aesthetics in a seemingly endless variety of aspects may be traced back ultimately to these four tap-roots in the sound wave. When that principle is applied, we experience an extraordinary simplification of the problem of experimentation in the psychology of music or musical aesthetics.

Let us take for example wave-frequency or, from the psychological and musical point of view, pitch. Absolute intonation such as is approximated by a resonated tuning fork is beautiful in certain relationships and is often an objective in music; but, as a matter of fact, it is seldom attained and for two reasons: first, failure on the ground of lack of ability in which case there is erratic deviation from true intonation, and we have elements of the ugly; and, second, artistic deviation from true intonation which is the medium through which the musician may express his individuality, his spontaneity, his inspiration, or whatever we may call those exhibitions of artistic power which lend grace to the tone in terms of pitch. This may take a great variety of forms such as various modes of transition from one note to another in attack and release, and numerous forms of periodicity, such as the vibrato and related means of expressing the feeling or in embellishments such as are characteristic of negro singing.

Now it is, of course, a commonplace that the exact nature of a deviation from true intonation can be measured and we may proceed to assort our samples into those deviations which result in ugliness

and those which result in beauty, and among each we may recognize quantitative features which enable us to express not only quality or form of beauty, but also degree of beauty or degree of conformity to accepted norms in each form.

The best illustration we have of this in the Iowa laboratory at the present time is found in the recent studies of the vibrato—that ethereal intangible medium for the expression of feeling, presumably so subtle that nobody could say exactly what it is. We can now define it in cold physical or mathematical language in terms of periodicity of pitch and of intensity of such and such form, such and such extent, and such and such frequency. We have collections of samples of the vibratos of the most famous singers and of various types of emotion and a large number of other conditions, and in every case we can show what is the prevailing type among the best singers, always stated with a precision characteristic of physical measurements.

We have then gone a step farther and produced synthetically a great variety of types of vibrato and degrees of vibrato with instruments in which all the factors are under control, and we have been able to get the judgments of distinguished musicians on what particular form of vibrato is the most beautiful for a specific purpose. Another interesting problem which has been settled by the same instruments is the problem of what we actually hear when there is a vibrato, because we have found that the listener does not hear rigidly what we find objectively to exist in the tone production. There are certain interesting laws of fusion which have been established by quantitative measurement. For example, a pitch vibrato of an amplitude of a quarter- to a half-tone is heard as having a specific or steady pitch. Is that pitch the mean, the upper, or the lower of the vibrato band? The answer may be expressed in terms of a number of laws of what Dr. Metfessel has called 'sonance', which is the analogy in the continuous tone to timbre in a cross-section of a clang.

From this single illustration in terms of pitch it may readily be seen how the principle applies to dynamic temporal or qualitative aspects of music as expressed in terms of the amplitude, the duration, and the form of the sound waves.

The fact that the hearing mechanism and numerous conditioning factors modify our experience, so that we are not justified in saying, for example, that the loudness of tone is always proportional to the amplitude of the sound wave, does not invalidate our proposition because such conditions simply challenge further refinement of meas-

urement so that these modifying conditions and conditionings may be taken into account in the formulation or interpretation of aesthetic principles.

I thought it worth while to make note of the hypothesis herein advocated because it brings great comfort and encouragement to the laboratory investigator in that he is starting out from a rigid and verifiable base which enables him to make comparable measurements and which aids him in making a complete objective analysis of a situation as complex as that of the attempt to formulate principles of beauty and ugliness in music in permanent language and in verifiable terms.

# PROTOPRAXIC AND EPICRITIC STRATIFICATION OF HUMAN ADJUSTMENTS

By Raymond Dodge, Yale University

## Introduction

Some time ago I witnessed the illuminating performance of a military drama in which a famous actor played the rôle of an outraged father. After denouncing a superior officer who had seduced his daughter, he suddenly drew a revolver and pointed it at the villain. Before he could pull the trigger, the consciousness of cast and military training took control of his behavior, and the unfired revolver fell to his side. The impulsive response to the relatively circumscribed stimulus-situation of outraged fatherhood was inhibited by a reaction to a wider stimulus-situation involving long training of responses to military cast.

The acting was good, the conflict was very realistically portrayed, and the sequence of impulse and control was clearly apparent. Meantime, the seducer stood like a wooden image—the unfired revolver was an every day occurrence in his experience. All primitive reactions to a possible threat in the situation were long since outlived, if they ever existed. His attitude plainly said to the audience, "Don't be alarmed, old dears, the man won't shoot, the threat is part of the play. I have been through it before and my murder is not in the book." I saw it as a moment of complete indifference, not one of the triumph of military training and bravery over self-protective instinct. The father's part was taken by a great actor, who had doubtless studied human conflicts and their portrayal, but the illusion he created was almost destroyed for me by the indifference of the supporting actor in what should have been a moment of tragic strength.

## The Problem

It is not difficult to detect an important psychological problem in the difference between these two types of acting. In this instance the better actor presumptively followed a real pattern of adjustment to situations as they develop while the other did not. In its widest formulation that problem involves the nature of interaction between

different strata of neural integration. Its full solution would involve a complete system of dynamic psychology which is far beyond our present experimental knowledge.

We are primarily concerned in this discussion only with a much more narrowly defined question which may or may not be a key to wider interpretations, *i.e.* what is the nature of that interaction between the protopraxic responses which we call reflexes and the higher systematizations which involve more highly elaborated data?

The instance of good acting which I have described adequately portrayed an inner conflict and competition of discrete and intelligible systems for the domination of behavior. There is relatively little mystery in that. The instance of failure is less obvious and more instructive. What is the difference between a convincing portrayal of triumphant bravery and acting that clearly says, "You and I know the revolver will not be discharged?" This question comes very close to our problem.

## Summary

The main points of this paper may be reduced to three, as follows:

(*1*) The traditional classification of human responses as reflex, instinctive, habitual, and voluntary, as though they were discrete levels of behavior correlated with supposed points of origin at various levels of the nervous system, involves a simplification fallacy of far-reaching influence on the understanding of actual human behavior phenomena.

(*2*) Our thesis, on the contrary, is that many, if not all, adaptive acts of the adult human are really complicated responses determined at various levels of integration. They commonly begin as a relatively crude approximate or protopraxic adjustment of short latency and develop into more adequate epicritic adjustments, according to the equipment of the individual, by stages that are often indistinguishable except in experimental situations.

(*3*) Our third point is more problematic and is presented only as a working hypothesis. Protopraxic and epicritic are not the names of entities of behavior, but are purely relative terms referring to greater or lesser elaboration of the sensory data. While our experimental discussion chiefly concerns the rôle of reflex protopraxic responses in total human adjustment, a considerable variety of beginning-responses may be regarded as protopraxic in contrast to those phases of response that occur after maturer elaboration. In this sense instinctive and habitual

protopraxias correspond schematically to the reflex phase as crude adjustments of short latency, which commonly pass into finer or epicritic adjustments to differentially elaborated data concerning the total environmental and purposive situation. Morals and even science itself represent strata of epicritic adjustment to which habit and impulse are protopraxic.

## The Traditional Doctrine of Psycho-Neurological Stratification

The doctrine of a stratification of neural systematization belongs to the relatively stable traditions of neurology. It is commonly held that each segment of the cord contains the central neural mechanisms for corresponding segmental reflexes involving not only the cells of the final common path to the muscles and the peripheral synapses of the afferent projection, but also at least one systematizing link, *i.e.* the intermediate neurones.

This segmental stratification is roughly shown by the retained reflexes of the experimental hind-, mid-, and fore-animal, and even more delicately by reflexes of experimentally isolated segments. Experimentally usable examples of segmental human reflexes are the knee-jerk, lid reflex and vestibular reflexes of the eyes. Since, however, the rest of the nervous system cannot be eliminated in normal human subjects, these reflexes are never uncontaminated by the action of other neural levels, so that true reflexes entirely free from complication of intersegmental influence can seldom or never be demonstrated in intact humans.

In addition to its peculiar functions as a neural center, each segment is traditionally held to be connected with many others as a link in the neural mechanism for the spread of efferent stimulation when impulses from the various segmental levels coöperate in or compete for the control of the final common path. Corresponding to the intersegmental connections of the neural strata it is customary to differentiate another kind of phylogenetic stratification of structure in which the autonomic system, together with the cord and brain-stem represent primitive or older systems, while more recent structures, such as the cortex, represent superposed additions to the primitive neural equipment. This kind of stratification corresponds roughly to the doctrine of neural levels, which is so frequently found in contemporary psychologies.

While the segmental stratification provides for the inheritance of discrete phylogenetic responses, the several neural levels provide for the integration of segmental responses into more complex reaction systems, reaching a suppositious upper limit in the reactions which are conditioned by the total experience of the individual.

In addition to the various phylogenetic stratifications, there is evidence in the tradition for something analogous to stratification in ontogenetic development, depending on the first appearance of specific responses in the life history of the individual. This ontogenetic development seems to be conditioned in the first instance by the successive myelinization of various parts of the embryonic nervous system. Anatomical stratification of post-natal neural development, if it really exists, is relatively obscure and complicated and can at present only be regarded as a more or less plausible hypothesis resting on certain characteristics of overt behavior and experience. For example, there seems to be some evidence for the ontogenetic fundamentality of certain early experiences, which may color the whole later life of the individual. Such influences may originate in early infancy and perhaps even in pre-natal experience. In the latter case they seem to join with phylogenetic systems in one continuous series. More superficial strata which originate in the relatively later life history of the individual show themselves in conditioned behavior and set, in memory and apperceptive systems, in habits and in behavior patterns.

For systematic purposes one may then distinguish two forms of pre-individual or phylogenetic stratification, *i.e.* segmental and intersegmental of the various neural levels. One may also distinguish various more or less problematic forms of individual or ontogenetic stratification of behavior and experience such as prenatal, infantile, adolescent, and sophisticated, reaching a relatively high form in science on the side of experience and purposive reaction on the side of behavior.

On the basis of comparative and genetic analysis of behavior, it is customary in psychology to distinguish typical forms of behavior which depend on the several phylogenetic and ontogenetic strata. Reflexes are supposed to originate at the level of the cord and brain stem, instinct in the basal ganglia, and experientially conditioned or purposive behavior in the cortex. Our psychologies seem to be widely committed to a hypothetical stratification of behavior corresponding with the various levels of neural systematization.

## The Interaction of Neural Strata

Along with the doctrine of the stratification of the neural systems and behavior, there exists also in our psycho-physiological tradition evidence for a more or less extensive interplay of neural action between strata and levels, with a consequent complication of behavior, so that a complete human response to any situation may be the consequence of widespread intersegmental stimulation. Under these circumstances an overt response might be properly regarded as stratified.

Traditionally the inter-relationship between the segmental strata in the central nervous system is that between a group of higher controlling systems and a more primitive group of systems whose action the former inhibit or reënforce. The final common path is known to be constantly subjected to impulses from various neural levels, so that any response originating at a reflex level is superposed on a complex dynamic background of pre-reflex conditions. There is experimental evidence that this generalization which was primarily found to hold true in animal experimentation also holds true for man. Our simplest reflex acts are seldom or never simple muscle twitches of uniform duration and extent. Each stimulus to a reflex operates on a neural center that is already more or less excited from the interaction of various levels. As it gains control of the final common path, the reflex stimulus never finds the system on which it acts exactly twice alike except by accident. For example, we have found experimentally that a voluntary deadening or enlivening of the knee-jerk may produce, respectively, depressed or exaggerated reflexes. The mechanism of such control of the lower strata by the higher is still a matter of more or less plausible hypotheses whose experimental investigation offers many difficult technical problems.

In the second place, the effect of any reflex stimulus may be prolonged through the action of intersegmental delay-paths into a series of efferent stimuli with more or less protracted contraction of the reacting muscle, resulting in a subsequent complication of reflex response by intersegmental action. Such response would be illustrated by the voluntary extension of the leg at the knee in reaction to the same stimulus that evokes a knee-jerk. The overt act in such a case originated as a reflex, but developed into voluntary behavior.

Presumptively, stratification of behavior is not an exclusion properly of segments or levels. Within the superior neural organ it

seems to take the form of a differentiation between systems of higher and lower excitability on the one hand and of physiological remoteness on the other. In any concrete afferent system the proximate cortical stratum seems to be the projection area. From that focus the spread of cortical excitation reaches widely separated areas which are related to the projection area in terms of greater or less systematic remoteness. This may be thought of as a kind of systemic stratification. Between these functional strata the processes of reënforcement and inhibition probably remain determinative and, in conjunction with the original stimulation from coöperating or competing afferent systems, probably control the extent and direction of the spread of cortical impulses.

Of these various forms of stratification only the segmental stratification has received adequate experimental verification. At the level of overt behavior it is still difficult to disentangle the intersegmental effects of the several levels and strata. We consequently still speak loosely of reflex, instinctive, and habitual acts as though the corresponding levels of neural integration were tight compartments.

Unfortunately, comparative and genetic analytical techniques are not commonly applicable to the analysis of overt adult behavior. Analogy tends to emphasize the segmental differences. Direct experimental analysis of the interaction of various strata in complicated overt behavior is, I believe, a desideratum. There seems to be at least one promising but as yet relatively unexploited method for such analysis—that is, the temporal separation of different phases of muscle contraction.

## Experimental Evidence for the Protopraxic and Epicritic Stratification of Adult Behavior

As far as I know, there are only two groups of published records of temporally stratified human responses when cortical reaction was superposed on a reflex act of the same mobile member. One of these was contained in my "Theories of Inhibition" (Part I, Plate 1, Records 1 and 2).[1]

These records show lid reflexes to sound with superposed voluntary closure of the lids. The matter seemed so important that a considerable number of similar records were subsequently taken from several

[1]Raymond Dodge, *Psychol. Rev.*, 33, 1926, 106-122.

subjects. All show the same characteristic dicrotism and discontinuous action-pattern. The first reaction consists of a reflex partial closure of the lid with a latency of 35–40$\sigma$, followed by a partial recovery, and then by another lid closure of cortical origin with a latency of from 90–150$\sigma$.

Differentiation of the temporal stratification of reaction is relatively difficult or impossible to observe and record in the movements of heavy limbs where the various phases of muscle contraction tend to fuse to a smooth line in the record, due to the physical characteristic of the motion of heavy bodies. They are more easily shown, however, in the records of corresponding muscle thickening. Similar dicrotism was reported in "Theories of Inhibition" in analogous experiments with the knee-jerk, except that in this case the records were made by the thickening of the quadriceps.

Records showing the temporal sequence of reflex and cortical control of the final common path to the external eye-muscles were published in "Adequacy of Reflex Compensatory Eye-Movements Including the Effects of Neural Rivalry and Competition" (Plate 3, and Records 3 and 4).[2] These records not only show the temporal primacy of the reflex movements of the eye and the delay of antagonistic cortical control, but they also differentiate the manner by which the protopraxic phase passes into the epicritic and reasserts itself when the data cease on which the epicritic control rests.

Another quite different set of records for the analysis of the overt behavior flux by the temporal sequence of protopraxic and epicritic muscle contraction phases resulted from an unpublished investigation of responses to pain. The general experimental setting was as follows. The subject was stimulated by sine-wave faradic currents of various more or less painful intensities. His reactions were recorded by a sensitive photographic system whose construction need not at present concern us. The essential feature is that it rested on the quadriceps muscles of both thighs and operated in such a manner that simultaneous thickness changes in both muscles neutralized each other. This automatically eliminated most of the effects of circulation and respiration, and recorded very fine differences in the relative tonicity. A telegraphic key in the hand of the subject provided him with the means of discontinuing the painful stimulation at any moment.

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[2]Dodge, *J. Exper. Psychol.*, 6, 1923, 169-181.

These experiments provided a considerable amount of data concerning the effect of pain on the tonicity of muscle, both neighboring and remote, and on the extent and latency of the reflexes which were simultaneously evoked. Only a small fraction of the data is relevant to the present discussion. With a suitable anticipatory set on the part of the subject quite painful stimuli could be endured for some time before the pain-producing electric circuit was voluntarily broken. Slight stimulation produced little or no effect on muscle thickening and was endured indefinitely. As the intensities increased, the first recorded effects were more or less widespread waves of muscle contraction with secondary superposed waves of shorter period. Whenever there was any effect at all on muscle tonus, it was always first in the direction of muscle thickening—that is to say, of muscle contraction. The effect on the reflexes was more complicated, depending on the temporal incidences of the respective stimuli. Even intensities evoking widespread waves of muscle thickening and reflex reënforcement could be endured for some time before the subject relieved the situation voluntarily by releasing the telegraph key. At high intensities of stimulation the practicable endurance was very short, and the antecedent muscle contractions were more sudden and of greater amplitude.

Two aspects of the experimental results are of especial importance in the present discussion. (1) As the effect of the pre-experimental set to endure the painful stimuli, there was a decrease of involuntary and crudely adaptive writhing and a delay of the more highly adaptive release of the telegraphic key. (2) There was the exaggerated temporal separation between the reflex of widespread muscle contractions and the more adaptive voluntary contraction which discontinued the painful stimulation.

Other records showing the sequence of protopraxic and epicritic phases in cortically initiated behavior are contained in the paper on the "Antagonistic Muscle Action in Voluntary Flexion and Extension" (Figs. 3a and 4a).[3]

These figures show how a protopraxic phase of co-contraction precedes and passes into the regular rhythmic series of muscle contractions and relaxations involved in the voluntary oscillation of a mobile member.

I believe that with proper techniques such sequences of adjustment will prove to be very common, if not universal phenomena.

[3]R. Dodge and E. A. Bott, *Psychol. Rev.*, 34, 1927, 241-272.

First comes an epicritic set for or against the reflex, modifying the resultant latency and amplitude of relevant muscle contraction. Then, with increasing intensity of stimulation, a greater insistence of the protopraxic response is finally continued into a more or less complex phase of epicritic adjustment.

## The Behavior Flux

While experimental data are still inadequate, I conjecture that much the same scheme may be found in so-called habitual acts, and that there are, in adult human behavior at least, no pure habitual responses uncontaminated by reaction set and epicritic elaboration of the sensory data. The degree of that elaboration is limited only by the capacity of the organism. Its native equipment and experience determine the epicritic reaction so far as time is available before the response to one change in situation passes into response to another.

Our picture of human adjustment is not a mosaic of reënforcing or conflicting reflexes, instincts, habits, and voluntary acts or a succession of discrete responses under these various categories, but a dynamic continuum, a sort of spiral process with a relatively simple front at any given moment and a highly complex background. Adequate experimental analysis should disclose that the front is really a succession of beginning reactions and elaborated adjustments. The beginning reactions are evoked by current stimuli superposed on the remains of consummated responses to past stimuli by which they are inhibited, reënforced, or qualitatively modified. Each protopraxic phase as it emerges is modified by the effects of more or less elaborated sense data from the same situation-stimulus, which in turn modifies the next protopraxic phase of response.

Our common experience is full of incidents in which the reaction passes from protopraxic to epicritic adjustment. When an automobile sounds its horn close behind one at a street crossing, the protopraxic start-reflex passes into successive stages of adjustment according to the elaboration of the sensory data. It may be quite different according to visibility, location of the automobile, the congestion of traffic, and the set of the victim. Elaborated response commonly begins by turning the head for more data in the estimation of relative distances. The consequent epicritic response may be any or all of a large number of reactions within the repertoire of the individual. He may stand still and raise his hand, run to one side or the other, according to traffic conditions and the estimated speed of the different

vehicles, or the distances of the sidewalk and safety zones. He may even take the number of the automobile, if conditions warrant it, and call for witnesses of violation of traffic regulations with a view to reporting the driver. The delayed and elaborated responses are relatively epicritic.

The more or less widespread muscle contractions of the protopraxic start-reflex may also be a common initial phase in all kinds of thwarted, non-adaptive, and mal-adaptive behavior, as well as for useful epicritic adjustments according to the violence of reaction, the configuration of the inhibiting and reënforcing stimuli, and the individual's capacity to organize them. If the epicritic adaptive adjustments are inhibited and the protopraxic responses intensified or reënforced, muscle co-contraction phase may pass into the complete paralysis of fright, into the rhythmic automatism of precipitate fright, or vituperation, according to the personal equation of the victim.

When the protopraxic phase falls in the field of habit, the possible illustrations are numberless. One starts to dress for some social function and suddenly finds himself preparing to retire. In the adaptation of speech and manners to a changed environment, any sudden emergency may evoke long-standing or habitual protopraxic responses and expose the normal social niveau and habits of the reactor before knowledge of the fitting or intended response can become operative. The old soldier who according to long psychological tradition disclosed his military training by dropping his bundles at an unexpected command to attention; the parvenu who lapses into habitual mannerisms as he tries to ape the manners and speech of a new culture; the tendency to lift one's felt hat by the brim after wearing a straw; colloquialisms, slang or grammar lapses; reaching for one's pipe in spite of New Year's resolutions; writing in familiar patterns in spite of intention to write better, or to disguise one's chirography all illustrate the outcrops of protopraxic response. Subsequent epicritic correction of these outcrops often serves only to call attention to the lapses.

## Conclusion

I venture the working hypothesis that all normal human overt adjustments to environmental changes include both protopraxic and epicritic factors. The hypothesis of stratified response is directly opposed to the doctrine that human instinctive acts are the product of

inherited psycho-physiological patterns. In adult humans, at least, there seem to be no instinctive patterns at all in the sense of uniformities of configuration of instinctive response. The primitive phylogenetic phase of an instinctively initiated act seems to be its protopraxic beginning, so that instinctively initiated response starts in phylogenetically predetermined directions, but the development and completion of such acts is an ontogenetic variable dependent on habit, experience, conditioning, and an understanding of the circumstances.

Similarly, our hypothesis denies necessarily mutually exclusive categories of instinctive and rational reactions. Probably all reactions have relatively protopraxic beginnings, but no adult human reaction except in extreme emotional inhibition is completed on the primitive level on which it starts.

Certainly it is not wise scientific terminology to name an act according either to its beginning or its terminal phase. For example, to classify any cross-section of the behavior flux such as eating, as reflex, instinctive behavior, or habitual action is misleading. In the ingestion of food there is an instinctive phase. There are also several reflexes and a great number of habitual and occasionally deliberative phases with clear purposive sets. The same analysis would be true of sex behavior, of combat, submission, and fright; in short, of practically all human behavior with instinctive protopraxic beginnings. Just as adequate particularizing description of the behavior would involve a qualitative and perhaps a quantitive statement of the sequence and interplay of these various phases, so adequate names for the behavior should not emphasize unduly any one of the several factors, but should somehow indicate their range and possibly their order. Thus a concrete case of fright beginning with a start-reflex and ending in complete intellectual paralysis might differ from another case which began without start in a gradual dawning consciousness of the frightfulness of the situation. The former might be called a reflex-emotional response, while the latter might be an inference-emotional-partially-adaptive flux. They are not merely qualitatively different degrees of fright. Specific cases may differ in origin and sophistication as well as more or less completely in sequence. Many of our actual episodes of emotion follow the former scheme. Many of our laboratory experiments in emotion follow the latter.

The formulae for human and possibly for infra-human behavior must involve an analysis of the behavior flux. Especially in individual

and racial differences one should describe the direction and duration of the several protopraxic reactions and the nature, competition, conflict and dominance of epicritic systems as the various strata emerge and succeed one another in overt behavior. In such description it is not enough to note the resultants. The main point would be lost unless the description included the temporal sequences.

The course of the succession is neither simple nor invariable. No two changes in situation are alike. Successive instances of similar situations never find the neural mechanism in identical conditions. Probably, also, there are complicating processes of degradation of reaction in which the epicritic phase of one stage of development becomes protopraxic in another, of refractoriness in which a differential duration of the refractory phase favors one kind of repeated stimuli over another, and of relative fatigue.

The grounds of this hypothesis are partly experimental and partly theoretical. Theoretically it is impossible to prevent the spread of afferent stimulation. The first phase of response may and probably generally does involve less elaboration than later phases. Conversely, those phases which are based on the greatest elaboration of the sensory data and on sophistication tend to come later in any concrete development of adjustment. It must not be supposed that we assume an exact correlation between elaboration and latency. Epicritic adjustment may appear in the form of anticipatory set, but there is a general tendency, consequent to each change in external vital conditions, which brings in the epicritic adjustments after some delay. Moreover, what appears to be antecedent epicritic set may on analysis prove to be the maturer elaboration of an antecedent protopraxic adjustment. The two are often indistinguishable in the behavior flux except on the basis of careful experimental analysis. Our thesis and hypothesis also rests on some experimental records of the reflexes. There is also, I believe, no inconsiderable support in other psychophysiological traditions which have led to various close approximations to our formulation.

Our working hypothesis relieves us of certain embarrassing categories and assumptions of invariant instinctive and habitual behavior patterns and furnishes a schema which is elastic enough to cover the description of a great variety of observations. In addition, certain apparent exceptions to the sequence of protopraxic and epicritic phases furnish a new viewpoint for the description of such anomalies as emotional reënforcement and hypnosis.

One can imagine a kind of general formula of behavior providing for the specific description of all muscular patterns, the time of emergence and duration of the several phases of an overt act, the set out of which they arise, their mutual conflicts and reënforcements as well as for a statement of the evoking stimuli. Until some such general formulae are available, corresponding in the field of behavior to Pavlov's analysis of the interconnection of stimuli, it seems a little premature to speak of a scientific behaviorism or, on the basis of what we now know, to arbitrarily neglect any phase of the complex behavior flux. Most of our present psychological descriptions of behavior patterns represent relatively narrow parts of such a formula. They seem to me to be altogether inadequate either for a description of the behavior flux in normal human adjustments, for interpreting their abnormalities, or for improving their adequacy.

Until closer approximations to such a general formula are available, we lack, I believe, the elementary foundations for a psychology of social relations, or a scientifically conditioned ethics. I conjecture, moreover, that the development of the individual or civilization by education is a task which will be found to be closely connected, not with suppression, but with the supplementation of protopraxic responses by suitable or desirable epicritic systems, as well as by conditioning the reflexes to favored stimuli. There seems to be no necessary antagonism between the two phases of adaptive adjustment, as is sometimes held. Both are commonly useful.

# A PROJECT FOR INVESTIGATING THE FACIAL SIGNS OF PERSONALITY

By KNIGHT DUNLAP, The Johns Hopkins University

Attempts to read 'character' from the face are of ancient origin, and apparently antedate the phrenologists' system of cranial diagnosis. In recent years, the so-called 'character analysts,' although retaining something of phrenology, and including in their systems old superstitions concerning the color and texture of the hair, have emphasized facial details strongly. It has been pointed out by many psychologists that these character analysis systems have no scientific foundation; and that they have no practical validity,[1] but this does not exclude the possibility that there may still be facial indications of something that may be called 'character' or 'personality.' For, as the psychologists have indicated, the essential feature, and essential weakness of the 'character analysts' theories is that they involve an attempt to correlate mental disposition with anatomical character, a correlation which does not exist in a mixed race. Physiological details, on the other hand, may have some correlation with mental traits, and any serious investigation of the physical signs of personality must concern itself with these. This subject, therefore, should be carefully distinguished from character analysis, and hence I suggest as a name for the topic under investigation, the new term, *idoscopy*.

From the visible activities and conditions of the face, we do judge certain temporary emotional conditions, such as anger, pleasure, fear, disgust, etc. My own investigations have shown these indications to be confined mostly to the muscles surrounding the mouth;[2] and various experiments by others have shown that the facial indications are by no means as accurate as have been supposed, and that our interpretations of facial expressions are influenced by many other factors. As regards individual differences, the situation may be more complicated still, and here we should expect the muscles of the upper part

[1]Knight Dunlap, Fact and fable in character analysis, *Ann. Amer. Acad. Pol. & Soc. Sci.*, 110, 1923, Nov.; *also* The reading of character from external signs, *Sci. Mo.*, 15, 1924, 153-165.

[2]Dunlap, *Genetic Psychol. Monog.*, 2, 1927, no. 3.

of the face to be more important than in the portraying of temporary changes of emotional conditions in a single person. This point, however, remains to be investigated.

'Personality,' a term used widely today, and used quite recklessly, needs defining. It is, of course, not an entity, but an abstraction, although a great many writers name and treat it as if it were an entity. I have defined it elsewhere, as the total or synthetic impression which another person has of the individual under examination. Or, we might say, the individual as estimated by another person. I have found no reason to modify this definition, and it is in fact being adopted by others. If we stick to this meaning, we shall prevent ourselves from entering into the realm of fiction, and fables. The exact relation of 'personality' to 'character' can be left to the readers' whims, with the suggestion that the latter term emphasizes the prediction of conduct more than does the former.

What man is, *per se*, can only be described in chemical terms, (and probably never will be). What we are interested in is how the man expresses himself, and further, the probability as to his future expressions. If we describe a man as 'honest,' we mean that his future action, or 'expressions,' will not be of the types that we describe as taking money illegally, deceiving, etc. What produces this probability, in the organism itself at this time, we do not know, and do not much care. What activities and stimulation of this part may have brought about this unknown condition, is an important psychological problem, which we may eliminate from the present discussion. Our problem at the moment is, whether the probability of certain practical expressions, which we call personality, may be indicated by facial details.

This possibility is raised by the consideration of the relatively long and slow development of individual tendencies. What a man *has* done in the past under certain conditions, establishes some probability that he will act the same way in the future. He may have changed his tendencies, but we can assume that these changes will not be sudden, and we can assume also that his past activities have left effects of various kinds on his organism.

Now, with responses in which emotional factors are entailed, we know that facial changes occur. The specific question confronts us, therefore, as to whether these facial activities have left marks or signs by which their past occurrences can be detected. If so, then these indications may be signs of what may be expected in the future.

Of course, past facial activities can not have modified either bone or cartilage. No changes in the general shape of nose, chin, ears, cheek bones, or position and size of the eyes can be expected. But there is a possibility that the muscles themselves may have been modified, and that the fascia, true skin, and perhaps other connective tissues may have been slowly modified spatially by frequently repeated patterns of muscular contraction. Hence, it is worth while to investigate the contours of the facial tissues, as indicated by what are called the "lines," which appear, sometimes with the face in repose, but more often and characteristically when speaking, laughing, or otherwise acting so that the face moves.

A preliminary study of facial lines, made by observing many individuals especially while they have been speaking, seems to indicate the hopefulness of this investigation. I have found it especially worth while to sketch, roughly, the faces of speakers addressing audiences or conferences, watching the lines as they appear and disappear, and putting the complete patterns in the sketch. From observations of this type interesting facts (or apparent facts) result. First, that each individual has a fixed pattern; secondly, that the lines of this pattern most often do not all appear at once, although all lines which do appear under various conditions, appear in the pattern; thirdly, that the facial patterns tend to fall into certain types of limited number. There are even certain apparent relations of type of line patterns to personality type, although this may turn out to be an illusion.

Here is a field for research not to be despised, and into which the present writer plans to enter next year. This outline is presented in the hopes that suggestions may be received from others before the work is too far under way.

Sketching of the pattern is useful for preliminary work, but not reliable enough for research purposes. Photographic methods seem more hopeful, and for this two techniques have been thought of. First, to coat the face thinly with a semi-plastic material, which shall in drying reach a stage in which it will crack nicely along the lines as they develop in the face as the person talks or otherwise 'expresses himself.' By illumination with light of proper wave-lengths, these cracks might be capable of sharp photographic registration. Secondly to pencil the lines *in situ* on the face with a crayon of proper con-

sistency and color, thus marking the pattern for photography. Both of these plans offer various difficulties and inconveniences, and it is possible that some other method may be devised.

The maximal difficulty will come when a sufficient number of maps are ready for study. Here is a matter on which great ingenuity needs to be exercised, and it seems possible that the ingenuity and experience of a number of persons will be needed.

Several suggestions have already been made by other persons. Dr. Bühler has suggested that in the case of women, the habitual repression of expression may produce line patterns differing from those of the male, in many cases, although those women who have not the 'defensive' attitude may show the other type of patterns. He has also suggested a study of the lines of the palm from the point of view of muscular tension—something entirely different from palmistry. Dr. Coover suggests that there might be a possibility of applying the lines of one person's facial pattern to the face of another for the study of the change in personality indication; a difficult technique, but not impossible.

Either a positive or negative result of the investigation will be welcome. But if negative, the results need to be especially clear and convincing. Hence, the especial need of coöperative work. The investigation is at least not chimerical, and all suggestions will be received with gratitude, and duly credited.

# SIX MORE PIDERIT FACES

By Samuel W. Fernberger, University of Pennsylvania

The present experiment is a repetition of the procedures used by Buzby,[1] and by Jarden and Fernberger[2] with the demonstration model for the Piderit faces devised by Boring and Titchener.[3]

The former investigators used only six faces of those possible—Anger, Dismayed, Horrified, Disdainful, Disgusted, and Bewildered. Buzby asked his *Ss* to pick the best descriptive term from a list of 18 such terms. No suggestion was given. He obtained a wide scatter of judgments and found a fairly small percentage of correct judgments. Jarden and Fernberger used the same six faces, but presented them with two degrees of suggestion. (1) The faces were correctly named and the *Ss* were asked to judge regarding the adequacy of facial expression. Under these conditions the number of correct judgments increased very materially. (2) The expression was analyzed and named and the face was built up before the *Ss* on the basis of this analysis.[4] This highest degree of suggestion somewhat increased the percentage of correct judgments for the relatively poor faces (poor on the basis of the Buzby scatter) but did not affect the percentage of correct judgments for the relatively good faces.

In the present study, six different faces were employed—Quizzical, Amazed, Contemptuous, Attentive, Stubborn, and Reverential. The same three procedures, used by Buzby, and by Jarden and Fernberger were again employed.

(*1*) *No suggestion.* The *Ss* checked what they considered the best descriptive term on a list of 18 terms which was furnished them in mimeographed form.

(*2*) *Named.* The *Ss* recorded (a) whether the face represented the expression named, and (b) the degree of excellence—using the following terms only, 'poor,' 'fair,' and 'good.'

(*3*) *Analyzed.* After the *Ss* had seen and listened to the analysis, they judged as above in (*2*).

An interval of at least a month intervened between each of the presentations. Results were obtained from the same group in every case, with absences which the experimentor could not control. For the first procedure (*No suggestion*) 770 *Ss* recorded results; for the second procedure (*Named*) 743; and for the third procedure (*Analyzed*) 724.

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[1]D. E. Buzby, The interpretation of facial expression, *Amer. J. Psychol.*, 35, 1926, 602-604.

[2]E. Jarden and S. W. Fernberger, The effect of suggestion on the judgment of facial expression of emotion, *Amer. J. Psychol.*, 37, 1926, 565-570.

[3]E. G. Boring and E. B. Titchener, A model for the demonstration of facial expression, *Amer. J. Psychol.*, 34, 1923, 471-485.

[4]For the exact wording of the instructions cf. Jarden and Fernberger, *op. cit.*, 565 f.

## Results

The results are given in Tables I and II. Table I contains the frequency with which judgments were made of every one of the 18 descriptive terms under the first instruction (*No suggestion*). The correct judgments are given in bold face type. We find the same scatter of judgments indicated by Buzby's study: for Quizzical, Amazed, and Reverential every one of the 18 terms was checked at least once; for Attentive and Stubborn 17 were checked, and for Contemptuous 16 were checked. The rank order of excellence, based on the frequency of correct judgments from poorest to best, is Amazed, Attentive, Quizzical, Contemptuous, Stubborn, and Reverential. The first three and the last three are very closely grouped.

Quizzical, Contemptuous, Stubborn, and Reverential were correctly checked more often than any other term in the list, but Horrified and Dismayed were checked more often than the correct Amazed; and Bewildered was checked more often than the correct Attentive. The highest percentage of correct judgments, which was for Reverential, was less than 50%.

Table II contains the percentage of judgments obtained under the two degrees of suggestion and under the instructions to report the different degrees of adequacy of the facial expressions.

The results of these two tables are shown graphically in Fig. 1. The percentage of correct judgments for the first procedure (*No suggestion*) is indicated by the shaded polygon; for the second procedure (*Named*) by the white polygon, and for the third procedure (*Analyzed*) by the black polygon. For the two degrees of suggestion, we have grouped the percentages of judgment for all three degrees of excellence (poor, fair, and good).

The results are very similar to those obtained by Jarden and Fernberger. In every case, the lower degree of suggestion materially increases the percentage of correct judgments. Under these conditions, the smallest percentage of correct judgments is for Attentive (75.5%), and the best is for Contemptuous (97.4%). Reports from some of the *S*s who refused to accept certain of the faces indicated that the slightly open mouth for Attentive was the basis for rejection and that the mouth for Reverential made the face almost a caricature.

For the higher degree of suggestion (*Analyzed*) our results are completely in accord with those of Jarden and Fernberger. This

increase in the suggestion leads in every case, to an increase in the percentage of correct judgments. The amount of increase is not great, however, except in the cases of Quizzical and Attentive. Under the presentation in which the face was analyzed, the author attempted to force the acceptance of the face to a greater degree than had been attempted by Jarden and Fernberger. In the case of

TABLE I

NO SUGGESTION
Frequency of Judgments

| Expressions Listed | The Expressions Employed | | | | | |
|---|---|---|---|---|---|---|
| | Quizzical | Amazed | Contemptuous | Attentive | Stubborn | Reverential |
| Pleased | 11 | 5 | 1 | 9 | 2 | 84 |
| Displeased | 33 | 20 | 20 | 7 | 92 | 14 |
| Stubborn | 17 | 1 | 9 | 8 | **372** | 13 |
| Attentive | 112 | 6 | 3 | **137** | 39 | 26 |
| Quizzical | **150** | 16 | 6 | 90 | 10 | 42 |
| Inattentive | 37 | 3 | 9 | 71 | 11 | 27 |
| Dismayed | 24 | 106 | 5 | 57 | 10 | 11 |
| Reverential | 10 | 2 | 2 | 34 | 17 | **379** |
| Affable | 27 | 4 | 3 | 35 | 3 | 48 |
| Bewildered | 65 | 101 | 2 | 180 | 1 | 29 |
| Amazed | 9 | **102** | 1 | 96 | 2 | 7 |
| Horrified | 3 | 181 | — | 4 | — | 9 |
| Raging | 1 | 4 | — | — | 1 | 4 |
| Disapproving | 53 | 22 | 72 | 10 | 112 | 24 |
| Disdainful | 132 | 23 | 198 | 13 | 30 | 20 |
| Anger | 6 | 5 | 3 | 1 | 24 | 2 |
| Contemptuous | 55 | 39 | **328** | 10 | 25 | 12 |
| Disgusted | 25 | 130 | 108 | 8 | 19 | 19 |

TABLE II

SUGGESTION: NAMED, ANALYZED
Percentage of Judgments

| Expressions Employed | Degree Expression Represented | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Good | | Fair | | Poor | | Not | |
| | *Named* | *Analyzed* | *Named* | *Analyzed* | *Named* | *Analyzed* | *Named* | *Analyzed* |
| Quizzical | 9.8 | 17.8 | 39.8 | 52.9 | 27.9 | 20.3 | 22.5 | 8.6 |
| Amazed | 33.5 | 26.5 | 33.5 | 35.2 | 15.3 | 29.0 | 18.0 | 9.3 |
| Contemptuous | 83.9 | 78.3 | 10.6 | 16.4 | 3.0 | 4.1 | 2.6 | 1.1 |
| Attentive | 21.5 | 43.1 | 33.6 | 37.6 | 20.3 | 14.8 | 24.5 | 4.6 |
| Stubborn | 56.3 | 52.1 | 29.6 | 34.0 | 9.3 | 10.8 | 4.8 | 3.2 |
| Reverential | 38.5 | 28.5 | 26.4 | 34.5 | 18.8 | 23.8 | 16.3 | 13.3 |

Attentive, for example, the author invented an alleged proverb that one could attend better when mouth-breathing in an effort to explain what had been described as the poor feature of this face. This proverb was told the group with a high degree of assurance and conviction and some reports show that it was widely accepted and, in some cases, even remembered as an old proverb! Under this highest degree of suggestion the best percentage of correct judgments rises to 98.9% (Contemptuous) while the worst is Reverential (86.7%).

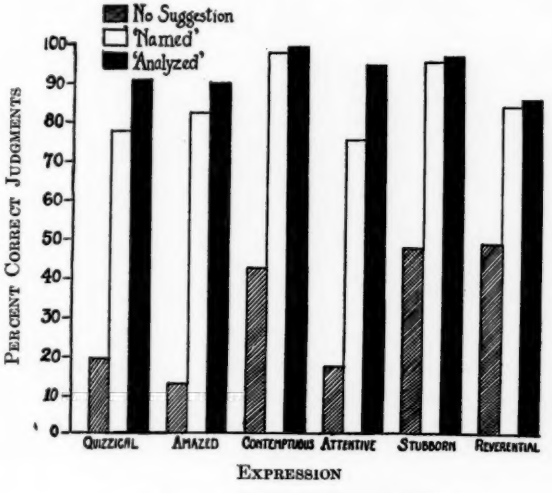


FIG. 1. Comparison of the results.

Although the analysis of the face increased the total number of correct judgments for every one of the six faces, it is of interest to analyze the results for the different degrees of assurance. Judged on the basis of percentage of correct judgments under instruction (*2*), (*Named*), the higher degree of suggestion (*Analyzed*) increases the number of good judgments for the two worst faces (Quizzical and Attentive). In the case of Attentive, the percentage of good judgments was more than doubled, once the mouth had been adequately but untruthfully explained. In the other four faces (Amazed, Contemptuous, Stubborn, and Reverential) the percentage of good judgments was in every case decreased by the complete analysis of the face.

No significant factors were found by an analysis of the results with regard to sex or with the degree of psychological sophistication as evidenced by presence in a first-year course or an advanced course in psychology.

## Conclusions

(1) The present results—for six new Piderit model faces—confirm the results obtained by Buzby, and by Jarden and Fernberger.

(2) There is a wide scatter in judgment of facial expressions when the *S* does not know what expression a given face is supposed to express. For Quizzical, Amazed, Contemptuous, Attentive, Stubborn, and Reverential, the percentage of correct judgments is in no case as great as 50 percent.

(3) Suggestion, such as the naming of the emotion and the analysis of the face, increases to a considerable degree the percentage of correct recognitions of the faces of the Piderit model.

(4) In all six faces, the higher degree of suggestion—represented by the analysis of the face—gave a higher percentage of correct judgments than the lower degree of suggestion (simple naming of the emotion). In no case, except Attentive, is the amount of increase of the percentage of correct judgments very much increased by the higher over the lower degree of suggestion. For Attentive, the increase is considerable due, it would seem, to the apparent explanation of a poor mouth.

(5) The conclusions indicated above are equally true for either sex, and for all degrees of psychological sophistication.

# LOCOMOTION AND RIGHTING MOVEMENTS IN ECHINODERMS, ESPECIALLY IN ECHINARACHNIUS

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## I. Introduction

Few free animals give the impression of greater inactivity than sand-dollars. Without obvious organs of locomotion their distribution in the shallow coastal waters where they occur might be expected to result from the direct action of waves and of currents. But anyone who is accustomed to collect these forms knows that though they are often thrown up in great numbers on sandy beaches when the seas are heavy they quickly disappear again under the sand where they are to be found resting in a characteristically horizontal position. My attention was first called to these peculiarities in making a collection of the common New England sand-dollar, Echinarachnius parma (Lam.), on Nahant Beach, Massachusetts. Subsequently I obtained ample living material at the Marine Biological Laboratory, Woods Hole, where I was enabled to study this species under very favorable conditions.

## II. Locomotion

If several live, vigorous sand-dollars are placed oral side down on the sandy bottom of a shallow aquarium, they will begin almost immediately to creep about, leaving behind them very characteristic trails in the sand. Two types of movement may readily be distinguished, rotation and progression. In rotation the sand-dollar may turn with its mouth as the center in either a clock-wise or a counter-clock-wise direction. A single individual may circle first in one direction and then in the other. This type of rotation has already been observed by Crozier[1] in the closely allied form Mellita.

Usually the movement of rotation is combined with that of progression; the sand-dollar not only swings round but begins soon to show rectilinear progress. After progression has thus been started the animal commonly maintains the newly established course for a considerable time. The rate of progression is not very great. The highest that I have observed was 18 mm. per min, and in a con-

[1] W. J. Crozier, Notes on the bionomics of Mellita, *Amer. Nat.*, 54, 1920, 436.

siderable number of trials carried out on six individual sand-dollars the average rate proved to be 13.7 mm. per minute. At such a rate a sand-dollar would cover the distance of a meter in a little less than an hour and a quarter.

Clark[2] has recorded somewhat higher rates for the apodous holothurian Synapta, namely, 20–30 mm. per min., and for the creeping comatulid Comatella, 85 mm. per min. This is a somewhat higher rate than that noted by me for the creeping of the large holothurian Stichopus parvimensis,[3] namely about a meter in 15 min., or roughly 65 mm. per min. None of these rates of locomotion approaches of course those seen in swimming echinoderms such as the comatulids some of which, according to Clark,[4] progress for the brief period during which they swim, at about 5 m. per min.

In Echinarachnius forward locomotion is accomplished in accordance with a definite structural axis. This axis extends from the anus which is on the margin of the animal's shell through its center near which the mouth is situated to the margin opposite the anus (Fig. 1). Although Echinarachnius is a radially symmetrical animal, many details in its structure, including the curves of its outline, the distribution of its spines and the like, are based upon a superimposed bilaterality of which the principal axis is that just described. In progression this axis is the line of motion and the edge of the disk opposite the anus is always carried forward; hence this edge is appropriately designated anterior, the anal edge being posterior.

If in the course of progression the anterior edge comes squarely against the wall of the aquarium or other like obstruction, the animal continues to work forward though without success and this futile activity may be maintained for some time. I have never observed a sand-dollar to back off from such an obstacle. Apparently a reversal of the normal direction of locomotion is impossible. After a period of ineffectual struggle against such an obstacle the sand-dollar usually escapes from the situation by rotational movements followed by locomotion in a new direction. In its inability to back away from an obstacle Echinarachnius is like Stichopus in which I have observed a similar incapacity for retrogression. Clark[5] has also noted that

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[2]H. L. Clark, The Synaptas of the New England coast, *Bull. U. S. Fish Comm.*, 19, 1901, 26 ff.; The comatulids of Torres Strait with special reference to their habits and reactions, *Publ. Carnegie Inst. Washington*, 212, 1915, 112 ff.

[3]G. H. Parker, The locomotion of the holothurian Stichopus parvimensis Clark, *J. Exper. Zoöl.*, 33, 1921, 206.

[4]Clark, *op. cit.*, *Publ. Carnegie Inst. Washington*, 212, 1915, 110.

[5]Clark, *ibid.*, 112.

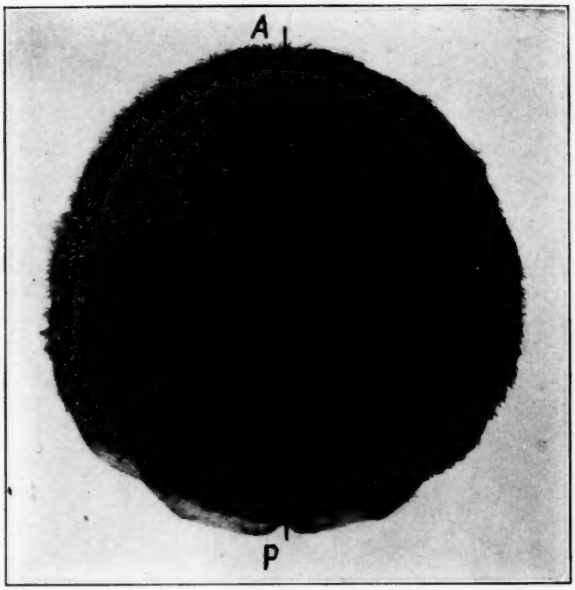


FIG. 1. Oral view of the shell of Echinarachnius parma. The axis of locomotion is marked by the black line; A, anterior, P, posterior. The mouth is central, the anus is posterior and marginal.

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certain comatulids are unable to reverse the direction of their locomotion and that they escape from situations such as I have described for Echinarachnius through rotation followed by progression in a new direction.

In straight-away locomotion Echinarachnius seems to be highly specialized, for it moves on only one structural axis and in only one direction on that axis. This likewise holds true for Mellita.[6]

Not only do clypeastroids exhibit this type of locomotion, but such a type probably also occurs among spatangoids. At Beaufort, North Carolina, I frequently dug the common spatangoid Moira atropos (Lam.) from its burrows, and I then had the opportunity of observing it in natural position. The burrow was so narrow that the animal could not easily have turned round in it. In all instances the long axis of the spatangoid agreed with that of the burrow, hence the direction of locomotion and the structural axis of the animal were the same. Further the animal was covered with spines which, like those of a porcupine, slope toward the anus. Such an arrangement would make it very difficult for Moira to move backward and I therefore believe that this form not only moves with its long axis parallel with that of its burrow but that it moves always with the anterior end forward; in other words it digs slowly head first. It would not be surprising if this type of locomotion were found to be characteristic of clypeastroids and spatangoids in general.

In the regular sea-urchins I know of no evidence to show that they are limited to any structural axis in progression or to one of the two possible directions on any given axis. Holmes in his account of the movements of Arbacia implies that this sea-urchin is free to move in any direction,[7] a condition which is certainly true for Centrechinus.[8] In such regular sea-urchins there does not seem to be even a physiological anterior like that found in a number of starfishes,[9] or in their isolated arms.[10] Comatula purpurea, according to Clark,[11] always moves with its long arms anterior and thus gives evidence of polarized locomotion such as occurs in Echinarachnius.[12] Thus the echinoderms exhibit all types of locomotion from

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[6]Crozier, *op. cit., Amer. Nat.*, 54, 1920, 435.

[7]S. J. Holmes, Phototaxis of the sea-urchin Arbacia punctulata, *J. Animal Behav.*, 2, 1912, 126-136.

[8]Parker, The geotropism of the sea-urchin Centrechinus, *Biol. Bull.*, 43, 1922, 374-383.

[9]L. J. Cole, Direction of locomotion of the starfish (Asterias forbesi), *J. Exper. Zoöl.*, 14, 1913, 1-32; H. P. K. Agersborg, Bilateral tendencies and habits in the twenty-rayed starfish Pycnopodia helianthoides (Stimpson), *Biol. Bull.*, 35, 1918, 232-254; W. J. Crozier, On the temporal relations of asexual propagation and gametic reproduction of Cosinasterias tenuispina, with a note on the direction of progression and on the significance of the madrepores, *ibid.*, 39, 1920, 116-129.

[10]A. E. Hopkins, On the physiology of the central nervous system in the starfish Asterias tenuispina, *J. Exper. Zoöl.*, 46, 1926, 263-275.

[11]Clark, *op. cit., Publ. Carnegie Inst. Washington*, 212, 1915, 112.

[12]Parker, The locomotion of the sand-dollar Echinarachnius, *Anat. Record*, 31, 1925, 332.

that in which there is complete freedom in the direction of movement to that in which both the axis of locomotion and the direction on that axis are rigidly circumscribed.

If a vigorously creeping Echinarachnius is closely inspected, its spines will be seen to be in great activity. They are certainly the chief organs of locomotion though they are without doubt supplemented by the ambulacral feet. Their activity can well be studied on the anterior edge of a creeping individual or still better on its oral surface. This surface can be inspected by allowing the sand-dollar to creep in an aquarium provided with a glass bottom and illuminated from below. By means of a mirror appropriately placed the activity of the ventral spines can be readily observed. It will then be seen that their movements coincide with the movements of the sand-dollar as a whole and that when they cease activity the sand-dollar ceases to move. The spines are developed symmetrically on either side of the locomotor axis of the animal and are largest near its anterior edge; they become smaller as the posterior edge is approached. The characteristic differences in the size of the spines and even something of their arrangement can be seen in preserved specimens (Fig. 1) where the contrast between the anterior and the posterior edges is most striking. In ordinary progression the spines work symmetrically on either side of the locomotor axis. In rotation the spines of the anterior half on both sides of the axis work together either vigorously toward the right or toward the left. These sets of movements imply systems of nervous coördination of no mean order and foreshadow conditions characteristic of the higher animal.

## III. Burying and Righting Movements

In heavy storms many sand-dollars are washed out of the sand and thrown indiscriminately on the beach. As long as they are covered with water they quickly dig into the sand again and disappear. Although they may be thrown one or other side down they all sooner or later bury themselves and when dug out of the sand they are regularly found in a horizontal position oral side down. Those that are thrown by the waves oral side up must therefore adjust themselves by some form of righting movement in burrowing into the sand.

The details of the burrowing habits of the sand-dollar can easily be observed in any aquarium with an ample layer of sand on the bottom. If in such an aquarium an active Echinarachnius is placed

oral side down, the animal rotates a little first to one side and then to the other in such a way as to work the anterior edge of its shell under the sand whereupon it begins forward locomotion with enough downward slope to carry it under cover. An ordinary Echinarachnius measuring approximately 5–6 cm. in diam. will thus bury itself in about 10 min. Mellita, which is somewhat larger than Echinarachnius, requires, according to Crozier,[13] at least 15 min. for this process.

It is a noteworthy fact that Echinarachnius will burrow in clean sand only. If an aquarium is allowed to stand without a sufficient circulation of water and the layer of sand on its bottom becomes slightly foul from the decomposition of the contained organic matter, an Echinarachnius will creep over the surface of such sand but will not burrow into it. If a sand-dollar is buried in such sand, it quickly emerges and remains indefinitely on the upper surface. The sensitiveness of Echinarachnius to the decomposition products in foul sand is probably due to its chemoreceptors. The presence of this type of receptor in the skin of such echinoderms as the starfish was long ago pointed out by Jennings.[14]

When an Echinarachnius is placed oral side up on a sandy bottom, a very remarkable response occurs. Through the action of the spines on the dorsal surface and the anterior edge of the animal it carries out a combination of rotational and progressional movements that sooner or later bring that edge slightly under the sand. The anterior edge is then very slowly and gradually worked into the sand while the animal as a whole is lifted from its prone position to, first, a sloping one and, then, a vertical one. This whole operation may require several hours (Fig. 2). Having attained a vertical stand the sand-dollar now rather quickly drops down with its oral side below the anterior edge meanwhile burrowing forward till the animal as a whole is carried under the sand. This turnover or somersault brings about a complete reversal of the sand-dollar in that at the end of the operation the animal has attained a position with its oral side down. In the quiet water of an aquarium while it may take as much as 3 hrs. for a sand-dollar to rise from the prone position to the vertical, it requires only about half an hour for the animal to drop from the vertical to the horizontal. This difference in the rate of the two steps

[13]Crozier, *op. cit.*, *Amer. Nat.*, 54, 1920, 436.

[14]H. S. Jennings, Behavior of the starfish Asterias Forreri De Loriol, *Univ. Calif. Publ. Zoöl.*, 4, 1907, 53–185.

of the process is due in part to the fact that the attainment of the vertical position is an operation carried out against gravity while that of the drop is a step favored by gravity.

A second factor leading to a difference in the two rates is the relative activity of the spines on the two sides of the animal. Those

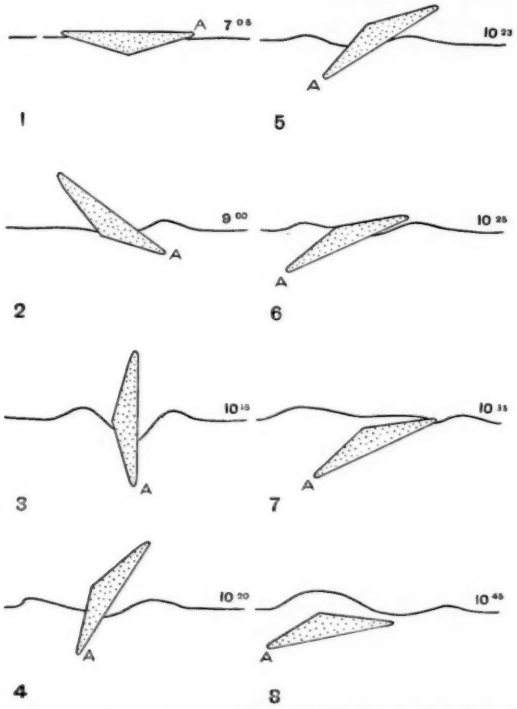


FIG. 2. Diagram illustrating from side view the steps in the righting movements of Echinarachnius parma. At stage 1 the sand-dollar is on the sand oral side up. At stage 3 it has attained a vertical position. At stage 8 it is oral side down and under the sand. The time of day for each stage is given to the right of each figure. The whole operation lasted from 7:05 A. M. till 10:45 A. M., or three hours and forty minutes. A, anterior edge of the sand-dollar.

on the aboral side are less developed and less active than those on the oral side and since the aboral side is more concerned with the rise to the vertical and the oral side with the drop to the horizontal, the first operation thus proceeds more slowly than the second.

When living sand-dollars are buried oral side up in deep sand and later examined they will usually be found all to have turned over. If they are buried only a few centimeters below the surface of the sand, the edges of their disks commonly rise above that surface as they revolve. An examination of these edges as they project from the sand shows the anus uppermost and thus indicates that righting movements under the sand are similar in type to those on the sand.

In one respect, however, these two types of movement differ. That under the sand takes place more rapidly than that on the sand. An inverted Echinarachnius on the sand will turn over in three or more hours. An inverted one under the sand will right itself in from an hour to an hour and a half. This difference in rate is due to the fact that in the buried state the sand in which the animal lies supports it more or less against gravity so that the retarding effect of this factor during the first half of the turn-over is much diminished and, further, the enveloping sand permits the spines of both faces of the sand-dollar to be simultaneously active instead of alternately so. Hence righting movements under the sand go on more rapidly than those on the sand.

It is evident from the preceding descriptions that the righting reaction of Echinarachnius is as circumscribed as its progression is. In the righting reaction, both on the sand and under it, the axis of rotation is always at right angles to the anterior-posterior axis of the animal and the direction of the turn-over is always with the anterior edge foremost. Hence in Echinarachnius the righting movement is limited as strictly as progressive locomotion is and in exactly the same way.

The righting movements in echinoderms have been a productive field of inquiry for a long time past. They were studied some years ago by Romanes and Ewart (1881),[15] Preyer (1886–1887),[16] Jennings (1907),[17] Glaser (1907),[18] Mangold

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[15]G. J. Romanes and J. C. Ewart, Observations on the locomotor system of Echinodermata, *Phil. Tran. Roy. Soc. London*, 1881, 829-885.

[16]W. Preyer, Über die Bewegung der Seesterne, *Mitth. Zoöl. Stat. Neapel*, 7, 1886–1887, 27-127, 191-233.

[17]Jennings, *op. cit.*, *Univ. Calif. Publ. Zoöl.*, 4, 1907, 53–185.

[18]O. C. Glaser, Movement and problem-solving in Ophiura brevispina, *J. Exper. Zoöl.*, 4, 1907, 203-220.

(1908, 1909),[19] and more recently by Clark (1915, 1917),[20] Moore (1920, 1924),[21] Wolf (1925),[22] and Yamanouchi (1926).[23] These reactions are inextricably interwoven with echinoderm geotropism which has been worked upon by Loeb (1891),[24] Clark (1898, 1901),[25] Baglioni (1905),[26] Mangold (1909),[27] Von Buddenbrock (1912),[28] Parker (1922),[29] and Bolin (1926).[30] Loeb discovered that the holothurian Cucumaria cucumis and the starfish Asterina gibbosa were both given to climbing upward on the walls of an aquarium. He believed that this was a response to gravity and he therefore described these animals as negatively geotropic. Similar observations were made on other starfishes by Mangold, and on sea-urchins by Baglioni, Parker, and Bolin. In all these instances the animals were believed to exhibit pronounced negative geotropism in that they crept upward through the water under the influence of gravity till they reached the top. There is very little evidence of positive geotropism among the echinoderms, but it is possible that under this head are to be classed the downward movements of Synapta as it digs into the sand, movements which have been described by Clark (1898, 1901),[31] and by Von Buddenbrock (1912),[32] and the similar movements of Caudina as reported by Yamanouchi (1926).[33]

Many negatively geotropic echinoderms such as starfishes and sea-urchins when they reach their upward limit in creeping come to rest without any characteristic orientation. A starfish, for instance,

[19]E. Mangold. Studien zur Physiologie des Nervensystems der Echinodermen, I, *Arch. ges. Physiol.*, 122, 1908, 315-360; *ibid.*, II, *op. cit.*, 123, 1908, 1-39; *ibid.*, III, *op. cit.*, 126, 1909, 371-405; Sinnesphysiologische Studien an Echinodermen, *Zsch. allg. Physiol.*, 9, 1909, 112-146.

[20]Clark, *op. cit.*, *Publ. Carnegie Inst. Washington*, 212, 1915, 97-125; The habits and reactions of a comatulid, Tropiometra carinata, *ibid.*, 251, 1917, 111-119.

[21]A. R. Moore, Stereotropism as a function of neuromuscular organization, *J. Gen. Physiol.*, 2, 1920, 319-324; The nervous mechanism of coördination in the crinoid, Antedon rosaceus, *ibid.*, 6, 1924, 281-288.

[22]E. Wolf, Physiologische Untersuchungen über das Umdrehen der Seesterne und Schlangensterne, *Zsch. vergl. Physiol.*, 3, 1925, 209-224.

[23]T. Yamanouchi, Some preliminary notes on the behavior of the holothurian, Caudina chilensis (J. Müller), *Sci. Rep. imp. Univ. Sendai, 4 Ser., Biol.*, 2, 1926, 85-91.

[24]L. Loeb, Über Geotropismus bei Thieren, *Arch. ges. Physiol.*, 49, 1891, 175-189.

[25]H. L. Clark, Synapta vivipara: a contribution to the morphology of echinoderms, *Mem. Boston Soc. Nat. Hist.*, 5, 1898, 53-88; The Synaptas of the New England coast, *Bull. U. S. Fish Comm.*, 19, 1901, 21-31.

[26]S. Baglioni, Über das Sauerstoffbedürfnis des Zentralnervensystem bei Seethieren, *Zsch. allg. Physiol.*, 5, 1905, 415-434.

[27]Mangold, Sinnesphysiologische Studien an Echinodermen, *ibid.*, 9, 1909, 112-146.

[28]W. von Buddenbrock, Über die Function der Statozysten in Sande grabender Meerestiere (Arenicola und Synapta), *Biol. Centralbl.*, 32, 1912, 564-585.

[29]Parker, The geotropism of the sea-urchin Centrechinus, *Biol. Bull.*, 43, 1922, 374-383.

[30]L. Bolin, Der Geotropismus von Psammechinus miliaris, O. F. M., *Intern. Rev. ges. Hydrobiol. Hydrogr.*, 16, 1926, 125-129.

[31]Clark, *op. cit.*, *Bull. U. S. Fish Comm.*, 19, 1901, 21-31.

[32]Von Buddenbrock, *op. cit.*

[33]Yamanouchi, *op. cit.*

on reaching the top of its climb may stop with its oral face down, to the side, or even up in that it may creep out on the under-side of any floating object such as a piece of wood. These animals therefore do not take up a definite position of rest in relation to gravity though they regularly creep against its pull.

Quite different from this is the condition seen in the sand-dollar. Here the animal can at best be said to possess only a slight positive geotropism which ceases to show itself as soon as the creature is under the sand. In this respect it is like Synapta which,[34] after it has made its way below the sand, rests quietly instead of continuing to show evidence of negative geotropism by deeper digging. According to Von Buddenbrock when the whole Synapta is covered with sand the stimulation produced by the contact between this material and the general surface of the animal inhibits further positive geotropism.[35] This probably also occurs in Echinarachnius for no sooner is it under the sand than downward digging ceases. But Echinarachnius, unlike Synapta, comes to rest below the surface of the sand with a very precise orientation to gravity in the sense that the plane of its flattened body is kept horizontal or at right angles to the pull of gravity and its oral side is always down. In this respect it agrees with Caudina which, according to Yamanouchi,[36] commonly rests under the sand with a definite orientation, that is, with its genital papilla uppermost and will somersault to attain this position.

The righting movement by which Echinarachnius attains its characteristic position when by accident it is turned over has already been described. The orientation of the resting sand-dollar can hardly be named in terms of conventional geotropism and yet no one will doubt that this orientation is determined by gravity. The condition here presented is precisely analogous to that in the great majority of higher animals in which the so-called normal position is maintained with the venter toward the center of the earth and the dorsum away from it, and yet the animal may not move either toward the center of the earth or away from it.

These various instances make it clear that the echinoderms show a wide range of gravitational response; from forms like the regular

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[34]Clark, *op. cit.*, *Mem. Boston Soc. Nat. Hist.*, 5, 1898, 53-88; *op. cit.*, *Bull. U. S. Fish Comm.*, 19, 1901, 21-31; Von Buddenbrock, *op. cit.*, *Biol. Centralbl.*, 32, 1912, 564-585.

[35]Von Buddenbrock, *op. cit.*, 585.

[36]Yamanouchi, *op. cit.*

sea-urchins which exhibit little other than a pure negative geotropism in that they creep upward orienting to the surface over which they move chiefly through touch, to sand-dollars which exhibit almost no geotropic reaction as such but which right themselves to the pull of gravity with great precision. This variety of conditions, some of which recall what is characteristic of the simpler types of organisms, plants included, and others of which foreshadow what is found among the higher animals, is perhaps what might be expected from the representatives of a group whose systematic position is intermediate between that of the simpler and of the more complex organisms.

Notwithstanding the great difference between the responses of the regular sea-urchins and of the sand-dollars it is evident that gravtiy is the all-pervading agency in both types of reaction. How gravity brings about the observed movements, particularly in Echinarachnius, cannot be stated with certainty. Sand-dollars will turn over in the dark as completely and as rapidly as in the light, thus showing that light though a stimulating agent for the skin of many echinoderms is not a necessary factor in this reaction.[37] The idea that the so-called negative geotropism in echinoderms is really a response to the concentration of oxygen in the water, supposedly greater toward the upper layers than in the deeper parts, has been definitely disproved by Mangold,[38] and Bolin.[39] Moore[40] attempted to show that the righting reactions of starfishes were dependent upon the stereotropism of their ambulacral feet, but Wolf[41] pointed out that this reaction was commonly initiated by movements of the arms before the feet had touched the substrate and that, therefore, pedal stereotropism was not essential to the turn-over. Mangold's view that the righting movement was started by the stimulation of the receptors on the dorsal face of the starfish through pressure when that face rests on the substrate, was shown by Wolf to be only partly correct. For if a starfish from which the dorsal integument has been removed is given time enough after the operation, it will carry out, contrary to Mangold's original opinion, complete righting reactions.

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[37]H. Plessner, Untersuchungen über die Physiologie der Seesterne, *Zool. Jahrb. Abt. allg. Zool. Physiol.*, 33, 1913, 361-383; C. Hess, Untersuchungen über den Lichtsinn bei Echinodermen, *Arch. ges. Physiol.*, 160, 1914, 1-26.

[38]Mangold, Sinnesphysiologische Studien an Echinodermen, *Zsch. allg. Physiol.*, 9, 1909, 112-146.

[39]Bolin, *op. cit.*, *Intern. Rev. ges. Hydrobiol. Hydrogr.*, 16, 1926, 125-129.

[40]Moore, *op. cit.*, *Gen. Physiol.*, 2, 1920, 319-324.

[41]Wolf, *op. cit.*, *Zsch. vergl. Physiol.*, 3, 1925, 209-224.

Through operational procedure Wolf was led to believe that the righting reactions of the forms that he worked with were largely dependent upon the strains put on the mesenteries in consequence of the weight of the digestive organs supported by these membranes. By an ingenious modification of Kreidl's experiment with iron filings on the statocysts of shrimps, Wolf demonstrated changes in the righting reactions of starfishes and brittlestars when their digestive organs containing iron filings were acted on by a magnet. Since these reactions were not so pronounced in starfishes from which the dorsal skin had been removed as they were in those in which this skin was intact, Wolf concluded that pressures on the dorsal skin as well as the mesenterial strains were concerned with the righting reaction.

Bolin has very recently advanced the view in the case of the sea-urchin Psammechinus that the bodily weight of this animal pulling on its ambulacral feet is the stimulus for its geotropic response.[42] He supports this view by changing the specific gravity of Psammechinus through the substitution of air for some of its bodily fluids. Under such circumstances the geotropism of the sea-urchin changes, and he concludes from this that the mechanical strain on the ambulacral feet is significant for the geotropism of this form.

I have not been able to test on Echinarachnius the views put forward by Wolf and by Bolin. But I have experimented with this form on other aspects of the general problem of geotropism. Echinarachnius, like other sea-urchins, possesses a certain number of sphaeridia. These have long been suspected of being concerned with positional orientation. The sphaeridia of Echinarachnius were first described by Lovén (1874).[43] They are five in number situated on the edge of the peristomal margin of the shell, one at the end of each ambulacrum. They are most conveniently placed for easy operative procedure. With the point of a fine scalpel they can be readily cut out without serious injury to the sand-dollar.

For experimental purposes all five sphaeridia were removed by the method just described from five sand-dollars. Five other sand-dollars were prepared as checks by making five slight cuts on the peristomal margin, one at the end of each interambulacrum, care being taken not to injure the intervening sphaeridia. The cuts made in the check individuals were quite as considerable as those inflicted

[42]Bolin, *op. cit.*

[43]S. Lovén, Études sur les Échinoidées, *Kong. Svenska Vet. Akad. Handlingar*, II, 1874, 1-91.

by the removal of the sphaeridia from the experimental animals. Both sets were then tested for their ability to perform righting reactions. The two lots were tried out in separate but adjacent aquaria. Both lots were first put upon the sand oral side down. After 5 min. all individuals had begun to bury themselves and at about equal rates. Both sets were then buried under about 2 cm. of sand with their oral sides up. In 20 min. all ten individuals were approaching the vertical position in the righting reaction and their posterior edges were above the sand, but the individuals whose sphaeridia had been removed were appreciably less advanced in righting than the checks. This was likewise true at the completion of the turn-over. The same test was twice repeated in the course of the day on these two sets of animals and in each test the sand-dollars without sphaeridia lagged behind those with these organs. Subsequently several other less regularly conducted tests were made upon different lots of sand-dollars with and without sphaeridia. Occasionally those without sphaeridia failed to turn over but they almost always did so though with characteristically slower and less accurate movements. From these observations I conclude that the sphaeridia of Echinarachnius are concerned with the righting reactions of this animal, but that they are not the only receptors thus concerned.

In this my observations and conclusions agree with those of Delage[44] and of Bolin[45] both of whom found that after the removal of the sphaeridia from the sea-urchins with which they experimented (Echinus and Strongylocentrotus in the case of Delage, and Psammechinus in that of Bolin) these animals still responded to gravity though with greater slowness than before the operation. It is obvious from these results that beside the sphaeridia other receptors are concerned with the responses of these animals to gravity. What these other receptors are I am not in a position to say. It is not improbable that the skin plays an important part in this activity and it is quite possible that the tension on the ambulacral feet, as maintained by Bolin for Psammechinus or on the supporting membranes of the digestive organs as demonstrated by Wolf, may be involved. Man maintains his upright position through a variety of receptors; the eyes, the ears, the organs of touch and of the muscle sense are all concerned. The condition in the echinoderms is obviously similar though probably not so complex.

[44] Y. Delage, Sur les fonctions des sphéridies des Oursins, *Compt. rend. Acad. Sci. Paris*, 134, 1902, 1030-1033.
[45] Bolin, *op. cit.*

## IV. Summary

(1) Echinarachnius parma on level sand will rotate with its mouth as a center in either a clock-wise or a counter-clock-wise direction.

(2) It will move on a straight course at an average rate of 13.7 mm. per min.

(3) The direction of straight-away locomotion is determined by a bodily axis passing through the centrally located mouth and the marginal anus. In locomotion the anal edge of the shell is posterior and the opposite edge anterior. The sand-dollar can creep forward on this axis but not backward.

(4) Creeping is accomplished by the movement of the spines supplemented probably by the ambulacral feet.

(5) Echinarachnius when oral side down will burrow into sand so as to cover itself in about 10 min.

(6) It will not burrow into foul sand and, if buried in such sand, it will emerge and remain on the surface.

(7) When placed on sand oral side up, it digs into the sand with its anterior edge lifting itself till it is in a vertical position and then slowly dropping oral side down. Meanwhile it buries itself.

(8) The lift from the horizontal to the vertical requires as much as 3 hrs.; the drop from the vertical to the horizontal about half an hour.

(9) This righting movement takes place under sand in the same way as on sand except that the whole operation under sand requires from only an hour to an hour and a half.

(10) Under the sand Echinarachnius ordinarily lies horizontally with the oral side down.

(11) The burrowing of Echinarachnius into the sand is probably a reaction to gravity (positive geotropism), but this is apparently inhibited when the animal is covered with sand. It then orients by maintaining a constant relation to the pull of gravity; it rests oral side down.

(12) The five sphaeridia in Echinarachnius are concerned, but are not the only receptors concerned, with its righting movements.

*Postscript.* Since the completion of the present paper, two other contributions dealing with the geotropism of echinoderms have come to my attention.

Pearse[46] who worked on Thyone, and Olmsted[47] who studied Synaptula, have both recorded righting reactions when these holothurians were turned out of normal position; and Olmsted has further shown that Synaptula is unquestionably negative in its geotropism, an observation that suggests that the burying reactions of such holothurians are not to be explained as the results of positive geotropism.

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[46]A. S. Pearse, Observations on the behavior of the holothurian, Thyone briareus (Leseur), *Biol. Bull.*, 15, 1908, 259-288.

[47]J. M. D. Olmsted, The comparative physiology of Synaptula hydriformis (Lesueur), *J. Exper. Zoöl.*, 24, 1917, 333-379.

# A PROGRAM OF ANTHROPOID RESEARCH

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## I. Introduction

"But what could you find to do with them for so long a time?" It was the query of a scientist friend on hearing me speak enthusiastically of devoting the remainder of my life to anthropoid research, and of hoping to leave to my successors a well-established anthropoid breeding and observation station in which the usefulness of the great apes as contributors to human knowledge and welfare might long continue to increase. Needless to say my questioner was not the author of "The Animal Mind," whose distinguished services to Psychology it is our privilege to recognize and commemorate in this volume.

The incident referred to occurred some three years ago. This is my "delayed response"! I trust that the delay has been justified by growth of my pertinent knowledge and insight. Certainly my available intellectual assets have been considerably augmented by intimate acquaintance with orang-outan, chimpanzee, and gorilla subjects of psycho-biological studies and by steadily increasing familiarity with the entire relevant behavioral literature. To-day I should modify my earlier seemingly exaggerated statement by adding the wish that my lifetime in anthropoid research might be as long as that of Methuselah, for even so I could not complete the program which I am about to present.

## II. Why Anthropoid Research?

From any point of view except that of the special pleader for the disinterested work of 'pure science,' a proposal for extensive anthropoid research demands practical and logical justification. I therefore shall prepare the way for a program by attempting to answer questions of the 'why' category as contrasted with the 'what' and the 'how'. If I make short work of the task it will not be because I consider the inquiries trivial or am impatient of them.

Why study anthropoid apes is a particular form of the question, why study any natural object or phenomenon? It has been answered so often by word and deed that any intelligent and fairly well in-

formed member of a civilized group should be able to take sides strongly. Reasonably convincing, unless one is extraordinarily biased, is the reply: knowledge is infinitely more valuable than ignorance, whether the subject be ape or microbe, rock or molecule, electricity or nerve impulse, idea or emotion.

Very well, rejoins the questioner, granted the general argument, why study anthropoid apes instead of men? To this form of the why question I must answer: they are to be studied not primarily instead of but in addition to men, and more particularly, for fruitful attack on certain problems for which human subjects may not be used.

However satisfying this form of reply may be to the reader, the following more explicit and thoughtful formulation of the query is worthy of careful consideration and of more detailed argumentative reply. Why study anthropoid apes, or any other infrahuman primate, when there are so many idle and apparently next to useless human subjects at hand? Why, indeed!

As I see them, the affirmative facts and arguments fall into two groups: (1) those from availability and controllability; and (2) those from observational values. Although on the earth to-day men greatly outnumber anthropoid apes, the latter actually are far more available for many kinds of research. This is the case primarily because to the use of human subjects there are varied and often insuperable economic, legal, moral, and sentimental objections, restrictions, or barriers. Oddly enough, it appears that in our present civilization, availability and controllability of human and anthropoid subjects are not greatly affected by supply. For many types of psycho-biological inquiry, human subjects are readily available and are being used freely and effectively. But for others, and among them many which are fundamentally important for human welfare and progress, they may not be used under existing regulations and public opinion because of incalculable risks to life, health, longevity, normal development, social relations and values. Given a human infant as subject, one is limited on every hand in freedom of experimentation. Given instead an anthropoid ape, only humane treatment and economy of resources need be considered.

Indeed, so marked is the contrast in degree of availability and controllability of man and ape that the latter only may be even considered for use in varied studies in *genetics*—experiment by selective mating, cross breeding, artificial fertilization, etc.; *physiology*—studies involving operations on a living organism, and other radical

changes in the physiological economy; *neuro-* and *psychopathology*—experimental control of neural development, production of atrophy and hypertrophy, inducement of mental defects and diseases; *psychology* and *psycho-biology*—continuous, controlled observation of the origins, genetic relations and values of major aspects of experience and response; for example, those generally classified as nutritional, reproductive, linguistic; *sociology*—experiments involving prolonged isolation of the individual, segregation and control of social groups, analytical study and measurement of the values of the social stimulus; *pedagogy* or *experimental education*—radical analyses of motivation; modes of adaptation; inhibition, reënforcement or creation of behavioral patterns; suppression, intensification or creation of interests; trial of new and possibly dangerous pedagogical procedures; and *hygiene*—in the hygiene of sex, for example, experimental studies of life-history in isolation versus natural social environment; observation and control of sex impulse and its expressions; genetic study of sex and reproductive activities and experience throughout the life-history of the individual.

Public opinion, investigative wisdom and insight dictate that the anthropoid ape be used, initially at least, in preference to man for many of the lines of inquiry enumerated in the preceding paragraph. In most instances it is a sheer matter of necessity, since human subjects cannot be used. But in any case, use of the anthropoid subject will cost far less in dollars, laborious hours, anxiety, risk of social censure, and legal infringement.

Arguments from observational values, apart from the significance of availability and controllability, are typified by the following. The anthropoid subject is peculiarly useful for: (1) trial and development of investigative techniques which may later be rendered applicable to man; (2) tentative formulation of problems and their refinement or reformulation in the light of exploratory investigations—the whole procedure to be viewed as preliminary to definite attack on specific problems which promise to be as important for the understanding and control of human life as of that of the anthropoid; (3) discovery of new investigative leads and of modes of directing or controlling vital phenomena—as for example, in the experimental study of behavioral adaptations, motivation, sex impulse, etc.; (4) analysis of observational findings, and comparative study thereof for discovery of characteristics, genetic relations, developmental and evolutionary trends, and their biological significance.

Convincing though arguments from availability, controllability and peculiar observational values of the anthropoid ape versus man appear, the immediate values of anthropoid research for human psycho-biology must depend to a very considerable extent on the degree of relationship between ape and man. Hence the appropriateness of the question: Is the anthropoid ape the natural and logical substitute for the human subject, or might one better choose some more readily available and controllable infrahuman subject, such as the dog, cat, guinea pig, or rat? The answer is definitely indicated by well-established facts, for structurally, physiologically, and psychologically the great apes (orang-outan, chimpanzee, gorilla) far more closely resemble man than does any other existing animal. Consequently these creatures are the preferred substitutes for human subjects in inquiries which may not be carried on with the latter and which have as an objective the extension of knowledge and control of human life.[1]

To summarize the arguments for an affirmative answer to the question under discussion, the anthropoid ape is the natural and desirable substitute for man in many types of investigation at this time and in our civilization because of (1) superior availability and controllability, (2) superior resemblance to man structurally, functionally and presumably also experientially, and (3) consequent peculiar values of methodological, factual, inferential, and theoretical discoveries and formulations in anthropoid research.

The way has been prepared for the original question: What can or should be done in a lifetime of anthropoid research? A program therefore is in order. I shall present, briefly and schematically, the one which commends itself to me, leaving descriptive supplementation to the imagination of the reader.

## III. What to Do: A Program

This program of research has as its objective psycho-biological description of the anthropoid ape which shall advance our understanding of the characteristics, relations, and the conditions of vital phenomena, and serve as basis for intelligent, constructive efforts toward modification and control. It is not a general all-inclusive

[1]This is true even though we may not, without observation, infer that a principle or mode of control established for an anthropoid ape applies in precisely like manner in man. Inference is always from general similarity to corresponding special likeness.

plan of biological inquiry, but instead represents definite specialization of interest in problems of anthropoid behavior, mentality, social relations, education, and hygiene, with desire to use other and related interests and techniques as aids. Undoubtedly adequate provision for the furtherance of this limited and special research program would offer facilities also for the study of other aspects and relations of the anthropoids.[2] Actually, almost any important scientific approach to the study of an organism has its significant relations to psychobiology, and may therefore be considered as auxiliary or supplementary to this particular program.

Conspicuous among the supporting interests and techniques which logically constitute the setting of any program of psycho-biological research are the ecological, the morphological, and the physiological, each with its abnormal or pathological as contrasted with its normal materials of description. Briefly each of these three basic or supplemental investigative interests will be considered in its essential relations to the problems of anthropoid behavior and experience which are to be enumerated.

*Ecology.* Except as an organism is known and understood in its normal environment and in relation to the chief factors, aspects, and variables of that environment, it may not be safely used as object of special psycho-biological studies. Logically, then, as well as practically, the ecology of the anthropoid ape is a prerequisite to intelligently planned and safely conducted naturalistic or experimental study of ape behavior or experience in field or in laboratory. This particular dependence of anthropoid psycho-biology will receive further attention in the later section on the 'how' of anthropoid research.

*Morphology.* Because psycho-biological phenomena ordinarily, if not always, are associated with or conditioned by organic structure and its changes, morphological facts and methods may not safely be neglected by the student of activity. Particularly important as aids to anthropoid research, and as promising opportunities for notable discoveries of fact and improvements in technique, are experimental morphology, structural neurology, taxonomy, and anthropometry, all of which for present purposes may be considered branches of morphology.

Morphological data of classification or taxonomy are required by the psychobiologist because his subjects must be identified and described in terms of some accepted schema. Moreover, there is the possibility that behavioral and psychological facts may significantly supplement the data of structural taxonomy, or may even suggest new principles, methods, and schema of classification.

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[2]A more inclusive program has been presented by the writer in Provision for the study of monkeys and apes, *Science*, N.S., 43, 1916, 231-34; and in The mental life of monkeys and apes: a study of ideational behavior, *Behav. Mon.*, 3, 1916, 133-43. The present program is 'mine' only as proposal, for naturally many investigators will participate in it either coöperatively or independently.

Anthropometry is essential in connection with anthropoid research because a special technique for physical measurement of the anthropoid apes is requisite to supply facts for the description and identification of individual, sex, age, or developmental status, variety and species. Moreover, anthropometry should provide also data for studies in anthropoid growth, nutrition, variation, evolution, and resemblance to man—matters which as deeply interest and intimately affect the activities of the psycho-biologist as of the morphologist. Obviously a special anthropometry should be provided for in connection with any program of anthropoid research.

The psycho-biologist may not be also a specialist in the varied techniques and informational accumulations of morphology, but he must at least possess fundamentally sound training and be able to appreciate and utilize morphological methods, facts, and principles as required in the pursuit of his own special problems.

*Physiology.* Between physiology and psycho-biology there is no logical boundary; they represent variations in emphasis and interest. Ordinarily the physiologist is not investigatively interested in problems of experience; the psycho-biologist may be, or instead he may limit himself to the objectively describable facts of behavior. It would be inexcusable to omit physiological categories of inquiry from our program, since many psycho-biological problems involve questions of neural function, glandular activity, effects of internal secretions, and aspects of reproductive and nutritional processes.

What follows by way of outline of a program of research implies, then, ecological, morphological, and physiological support. Without it, psycho-biological study of the anthropoid apes would be relatively unprofitable. But on the other hand, the psycho-biologist may in his turn hope to render significant contributions to supporting divisions of biology.

It is proposed to present a program of anthropoid research by enumerating, with brief descriptive comment and illustration, thirteen major assemblages of psycho-biological problems, each of which involves behavioral as well as experiential aspects. Claim is made neither for completeness nor adequacy of description. The plan is dictated rather by practical than logical considerations, and one of its chief recommendations and merits is that it is even now in process of realization.

## Assemblages of Problems

(*1*) *Action system.* Elements, origins, relations; nature versus nurture; types, characteristics, and their significance. This category includes naturalistic and experimental study of the appearance of behaviorial elements, their maturation, association, modification, supplementation, change in senescence, and possible final disappearance; also cross-sectional views of behavior at various stages in the

life-history and development of the organism. The conventional categories of response, as Dodge[3] from his analysis of human reactions finds reason to maintain, are descriptive conveniences which often give an unjustifiable appearance of independence. Genetic description abundantly indicates the intimacy of relation of typical forms and phases of reflex, instinctive, voluntary, and habitual activities. Few responses, it seems, are first, last, and always classifiable in a single category. Instead, it is manifest that a given reaction may at various times in its history exhibit the characteristics of each type, and indeed appearing as a simple reflex may become in turn instinctive, voluntary, and habitual. Hence the importance of listing under the caption 'action system' problems of genetic relation. Given developmental history and adequate description of the anthropoid action system, there can be small doubt that our present technical terms for organic responses and our categories of classification will either acquire new meanings or fall into desuetude. The latter is the recent fate of the term 'instinct'.

Although the groups of problems which have been suggested are primarily descriptive, experimentation in connection with the anthropoid action system might very well be directed toward its modification or control.

(*2*) *Receptivity and sensibility.* Modes and qualities, their characteristics and development; life-history curves for the various forms of receptivity; measures of their relative importance, mutual relations and modifications. Systematic study of the general and specific relations of the organism to its environment is in point as essential preparation for effective attack on various other groups of psycho-biological problem. Until modes of receptivity in an organism are definitely listed, their characteristics discovered, measured, and reliably described, studies in motivation and behavioral adaptivity lack proper background. All of the problems of human sense-physiology, and many of those of the psychology of sensation, find legitimate place in anthropoid research.[4] Threshold, acuity, range, variation, fatigue, inhibition, reënforcement, supplementation, dis-

[3]Raymond Dodge, Protopraxic and epicritic stratification of human adjustment, this VOLUME, 145 ff.

[4]Some of these problems are fundamental in this program, others are of minor importance; some may more conveniently be studied on human subjects, others on anthropoid apes.

crimination, environmental dominance or relative importance, and time relations, suggest aspects of either receptivity, sensibility, or both, which may be dealt with advantageously. Nor should the abnormal and pathological variations be overlooked.

Finally, as a result of combined naturalistic and experimental, qualitative and quantitative investigation of this great assemblage of problems, there should become available reliable and reasonably accurate general description of the receptive capacity and characteristics of anthropoid individual and type. This achievement is much to be desired as basis for many other varieties of investigation. At present, psycho-biologists commonly assume in their infrahuman subjects certain modes and characteristics of receptivity, and in devising their techniques depend upon analogical inference or guesses instead of upon precise knowledge.

(3) *Expressivity and affectivity.* As in the case of the previous category, so in the present, informational needs and problems suggested thereby are legion. Among the conspicuously important groups of problem are modes and varieties of affective expression, their genesis, constitution, and relations; elements of affective expression, their mutual relations, values, and phenomenal association with such conventional affective categories as feeling, emotion, mood, sentiment. Behavioral pictures of types of emotion are important desiderata. Degree of emotionality and expressivity should be measured, and the life-history, variations and changes of affective patterns of response should be accurately observed and subjected to experimental analysis with intent to modify them. Relations of these categories of experience and expression to general docility and specific forms and conditions of behavioral adaptation, to factors of motivation, and also to neural and other organic structures and mechanisms, should be intensively studied.

It is the opinion of a competent observer that the chimpanzee more closely resembles man in its affective life (expressivity doubtless being meant) than in its intellectual or cognitive behavior.[5] Are affective experiences and their characteristic behavioral expressions primitive, and do they phylogenetically as well as ontogenetically antedate adaptive intelligence? What are the relations of affectivity, either experienced or expressed, to cognitive or intellectual forms of

[5] N. Kohts, *Untersuchungen über die Erkenntnissfähigkeiten des Schimpansen*, 1923.

experience and their expressions? Is motivation in the anthropoid ape, or indeed in any organism, primarily or exclusively affective?

(*4*) *Temporal relations of psycho-biological phenomena.* Temporal relations of neural processes and of mental processes; simple reaction-time for each mode of receptivity throughout its stimulus range and under varying external and internal conditions; discrimination time; complication, memory, ideation, volition, adaptation. Each of these psycho-biological terms suggests possible measurement of temporal relations which may prove valuable in the analysis and description of action system, behavioral patterns, and their modes of growth and modification. At one time the rage among psychologists, reaction-time studies have been replaced largely by other interests and techniques. Nevertheless, measurements of time-relations promise to be fundamentally important in studies of neuro-mental rivalry, inhibition, reënforcement, association, fusion, and integration. They may not be neglected when one seeks the genetic history of behavioral patterns and description of their mutual adaptations and elaboration into systems of behavior and conduct.

(*5*) *Perceptions, configurations, coördinations.* Genetic description of perceptual processes and their motor expressions may be approached by way of the analytical study of perceptual configurations, their elements, and the nature of the bonds which constitute them experiential or behavioral wholes. Little indeed is known even in man, still less in the anthropoid apes, despite the obvious importance of such knowledge, of the characteristics of configuration either (a) of like elements, or (b) of differing elements, or of their manner of growth, rôle in the life of the organism—in a word, of their life-history. Spatial and temporal relations, and relativity of configurational elements and wholes are problem-suggesting. Favorable approaches to problems of perception in the anthropoid ape are numerous and obvious. Already some have been indicated and the fruitfulness of research demonstrated.[6] Experimental procedures are at hand or may readily be devised, for in basic genetic investigations with the apes equivalents of many of the methods already applied to human subjects may be utilized, as well as others which for one reason or another may not safely be applied to man. Until the world of the apes is known in terms of its perceptual characteristics and values,

[6] W. Köhler, *The Mentality of Apes*, 1925.

we may not hope to understand their behavior or to work with them intelligently and fruitfully in studies of motivation, adaptation, and social relations.

(*6*) *Mnemonic processes.* Fundamental in this field of inquiry is knowledge of the nature of mnemonic process, as bodily change and as experience. The appearance, characteristics, and relations of imaginal experience and behavior demand attention. Peculiarly important and interesting groups of problem are suggested by the terms mnemonic, anticipatory, and constructively adaptive response or behavior. The investigation of mnemonic phenomena deserves conspicuous place in this program of anthropoid research because of the probability, indicated by recent discoveries, that the primates, and more particularly the anthropoid apes and man, are distinguished from other existing animals by the nature and functions of their mnemonic processes. In these organisms, it would seem, the image appears as essential condition of mnemonic, anticipatory, and ideational response.

(*7*) *Ideation, insight, thought, generalization.* Problems of ideation are numerous and insistent. Demanding first place are those centering about the appearance or existence of ideas and ideational behavior in the anthropoid apes. Given the phenomena, their characteristics and conditions of appearance should be investigated. The appearance, nature, and values of symbols; the characteristics of reflective experience and behavior, and of generalization, indicate avenues of approach to the study of intelligence and its expressions. The present is an era of the discovery of new approaches, psychological, behavioral, and physiological, to the identification and investigation of ideational phenomena. Progress in this field is relatively rapid, and increasingly important discoveries are forecast by current achievements. A more important assemblage of problems than those designated by the caption "ideation, insight, thought, generalization," would be difficult to imagine. And it would be difficult also to imagine more serviceable subjects of observation to supplement our human resources than the anthropoid apes. In them, if anywhere in the present fauna of the earth, we are likely to discover primitive forms of ideational experience and expression and the appearance of symbols and generalization.

(*8*) *Motivation.* Problems of motivation logically should take precedence over those of memory, ideation, and adaptation, since

their solution should prepare the way for study of the latter aspects of life. Actually, although it is difficult to believe or to understand, the incomparably interesting and significant assemblage of problems designated by motivation has been neglected by the experimentalist, and even now is commanding a disproportionately small amount of attention. The anthropoid apes present themselves as peculiarly fitting subjects for studies in motivation. Each of the following subgroups of problem may suggest a life-work!

The discovery and enumeration of motivational factors and conditions (physiological, affective, cognitive); observation of their relations and varying values. Qualitative and quantitative studies of motivating factors and complexes. The variation, range, optimal condition and modification of motivation. Ordinarily, motivation is complex; hence insistence on the distinction of factor or constituent and complex or totality of motivating conditions.

It is beyond argument that definite knowledge of motivation and the ability of the investigator to control as well as to describe it are prime essentials, thus far too largely ignored or neglected, in the quantitative study of habit formation or of any other aspect of behavioral adaptivity. Nevertheless, many of us psycho-biological investigators have inadvertently, ignorantly, or stupidly placed the cart before the horse and stubbornly refused to admit the illogic and wastefulness of our procedure.

(*9*) *Adaptivity and education.* Pertinent are the varied problems of 'habit formation' and 'learning' as exhibited in the literature of human and infrahuman psychology, and also many phases of the general problem of the genesis of individually acquired versus structurally given modes of activity. It is indicated by current research, the results of which are in the main unpublished, that behavioral adaptivity is qualitatively similar in man and in anthropoid ape, whereas in animals other than the primates the psycho-biological process is qualitatively less varied and differs also quantitatively. Therefore the unique theoretical importance of thorough-going study of modes, varieties, or forms of behavioral adaptation in the anthropoid ape and comparison of results with those obtained on the one hand with human subjects and on the other with monkeys. Whereas adaptation by insight or ideational process appears to be lacking in most animals, it evidently occurs in the anthropoid apes, and perhaps also in certain other primates.

Experimental education is likely to find in the anthropoid apes uniquely valuable subjects because of the peculiar advantages of availability, controllability, and observational values already enumerated. With them, new and daring pedagogical experiments may be made without embarrassment, and leads may thus be obtained for promising tests with human subjects. The experience of those who have worked intensively with types of anthropoid and who are intimately familiar with them, clearly enough indicates that the significance of these creatures for the promotion of education cannot well be exaggerated.

(*10*) *Language and symbolism.* In this category the fundamental problem is that of the existence, nature, manner of origin and development of symbolism in experience and behavior. Among linguistic problems, which as a group imply the presence of symbols, there are two conspicuously important sub-groups: (a) those of vocalization and speech; and (b) those of bodily attitude, posture, and movement (especially gesture). Species intercommunication in the anthropoid apes appears to depend primarily on these forms of behavior; thus both affective and cognitive experiences are behaviorally transmitted and transferred. Doubtless language in this inclusive sense, and ideation, thought, generalization, will on examination prove to constitute a homogeneous assemblage of problems, whose solution in turn will illumine many aspects of social relations and values.

(*11*) *Norms and differences: individual, sex, age, species, environmental.* Naturally this group of problems cuts across the categories already proposed, for differences may be discovered and norms established in connection with any psycho-biological phenomenon. Aside from the achievement of practically serviceable norms for such phenomena, determination of their variability and of the stability and conditions of 'differences' would seem especially desirable. Doubtless it will become increasingly obvious as this program of research is carried out, that individuality is of paramount importance and may not safely be neglected by the investigator. Indeed, it already is evident that in individualization the orang-outan, chimpanzee, and gorilla closely approach man. All fruitful comparison of psycho-biological data is conditioned by the availability of reliable

norms and measures of difference descriptive of individuality, the sexes, developmental stage, status, or age, species or organic type, and ecological or environmental situations or relations.

(*12*) *Social relations.* Beginning with the ecological, this category extends into all groups of problems which involve or are conditioned by the social stimulus. A primary task in this investigative field is the measurement of the value of the social stimulus under certain carefully chosen and standardized conditions. With respect to this value, the genera and species of anthropoid apes and man would appear to differ no less markedly than do individuals. Clearly the social stimulus functions in perception, memory, ideation, in many forms of adaptation, language, and in various situations which do not necessarily involve these forms of experience or expression. It may be discovered that the isolated individual differs radically in reactive capacity from the member of the social group. The words imitation, sympathy, aid, coöperation, tradition, custom, institution, suggest phenomena which are as open to scientific study in the anthropoid ape as in man.

(*13*) *Conduct and morality.* The emergent, developing, or established system of activities to which the term conduct is applicable, and in connection with which the genesis and evolution of morality may be discovered, are more readily observable, because simpler and more primitive, in the anthropoid apes than in man. Sociologist and student of ethics will be alert to this novel observational approach to certain of their difficult problems. A naturalistic ethics is more likely to be achieved through the utilization of the infrahuman primates, beginning with the anthropoid apes, than in any other manner. Suggested by observations already on record are studies in social regulation, as for example, by taboo, communistic and individualistic tendencies, sympathetic coöperation, forms of dependence, and altruistic attitude.

## Characteristics of the Program

These thirteen categories, chosen deliberately to constitute an attack at once practicable and logical on fundamentally important psycho-biological problems, instead of exhausting the possibilities of fruitful inquiry, merely typify and by example illustrate problems whose solution may reasonably be hoped for through systematic, sustained, and technically ingenious use of anthropoid subjects. It

remains to characterize the proposed program briefly from the standpoint of ideal and intent. As conceived and in course of realization it is (1) inclusively coöperative and unitary; (2) historical;[7] (3) correlational; (4) open-minded and hospitable, though critical and questioning; (5) proceeding from the known to the unknown; (6) experimental and pioneering; (7) idealistically pragmatic in theory and practice; (8) eudemonistic.

These several descriptive terms demand comment, if not also explanation. Inclusiveness involves the ignoring of the artificial boundaries of the physical and biological sciences, and the pursuit of problems in accordance with technical and informational requirements. The plan is historical in intent because genetic description of psycho-biological phenomena is a primary objective and, by assumption, a logical approach to the understanding of behavior and experience, and to achievement of serviceable measures of control. The undertaking is necessarily correlational because structure, activity, and experience—appearing, adapting, developing, disappearing—constitute the varied materials for unitary description and the formulation of laws. Although questioning the adequacy of existing methods and the reliability of much of our present information, and seeking especially to eliminate the influence of superstition, prejudice, and outworn scientific procedures, the program is by intent hospitable to all ideas. The lesson for this formulation is taken from the dominant influence of religious and other social conditions and institutions on the development of man's knowledge of the life of anthropoid apes. At every turn affective attitude, preconception, desire, prejudice, have influenced observation and inference.

That in execution of the program procedure should be from the known to the unknown would appear inevitable, yet careful reading of the history of science indicates the prevalent tendency to neglect historical background and to proceed as though the individual observer were the original seeker for truth. That the plan is primarily experimental does not entail the exclusion of other methods of discovering facts. It does however involve the extension of experimental methods of inquiry, their refinement and supplementation, as well as similar improvement of naturalistic developments. Especially are technological innovations and new, novel, ingenious proce-

[7] The logically initial task of surveying, digesting, and summarizing the behavioral literature on the anthropoid apes, and of preparing for publication a general historical and descriptive account of the subject, approaches completion.

dures essential for the development of such aspects of the program as the sociological, pedagogical, and ethical. Three assumptions justify the characterization "idealistically pragmatic:" (a) only the truth is worth knowing; (b) any question which can be intelligently asked is answerable; and (c) in research, faith in the attainment of the seemingly impossible is essential to progress. Similarly, use of the ethical term "eudemonistic" is based on assumption of the supreme worth of human happiness and definition thereof in terms of service for human welfare. Further, the acquisition of the material is recognized as a minor 'good,' and 'getting' merely as an essential condition of 'giving.'

## IV. How Proceed?

The conduct of this program of anthropoid research involves the use of three major types of situation which differ mainly in their environmental characteristics, technical facilities, and in the relations and conditions of the anthropoid subjects. They may be designated briefly as (a) field; (b) breeding and observation station; and (c) special laboratory. Each must be described somewhat more fully in order that its observational values and other significance for the program may appear.

(*1*) *Field.* The term implies naturalistic study of the daily life, ecological relations, life-history, adaptations, and especially the social relations of the anthropoid apes in the freedom and fitness of their normal habitat. Many problems may thus be studied advantageously, but many others may not. Field observation undoubtedly should supply basic data on the characteristics of an animal to guide and check the experimentalist in evaluating the behavior of like subjects in the unfamiliar, if not also grossly unnatural, conditions of laboratory experimentation. It is beyond denial that every subject of laboratory or other experimental investigation should be carefully described psycho-biologically and understood in the light of adequate knowledge of the normal, free, natural life of the species. The experimentalist who in working with anthropoid apes ignores this informational safeguard and form of preparedness, risks misleading results and misinterpretations.

As I write this paragraph I am reminded of a comparison of naturalistic and experimental method which I wrote nearly twenty years ago, and instead of rephrasing the ideas I beg to be allowed to quote two paragraphs.[8]

[8] Scientific method in animal psychology, *Extrait des Comptes rendus du VI Congres international de Psychologie*, Geneva, 1909, 809-810.

"The history of our science—or part science, if you prefer—may be briefly told. Beginning with the purely naturalistic observations of a generation ago, it rapidly passed through the stages of anecdote and naturalism to the stage of careful and critical observation under experimentally controlled conditions. Credence is no longer given to animal stories unless they are verifiable by available scientific methods. For the progress of our science, this reaction against the anecdote and the prejudiced story of the lover of pets was necessary; but it has been carried too far. To refuse to believe statements simply because they can be verified only by the sympathetic observer of an animal is folly. It is high time that we experimentalists awoke to the fact that other animals are like us in that they are totally different creatures in the face of different situations. My dog may seem stupid indeed in the strange and mysterious atmosphere of your laboratory, whereas in the familiar environment of my study he appears to be surprisingly intelligent. Lkie the child, he shows off to advantage only under certain favorable conditions. Thorndike's famous experiments with cats and dogs, and my tests of the intelligence of frogs and tortoises, show what these animals will do under certain unfavorable experimental conditions, not what they are capable of doing when they feel at home in their environment.

"Just because animals can not be studied profitably unless their natural adaptations to environment are thoroughly known and intelligently considered, I am led to distinguish two classes of methods in Animal Psychology. They may be called the *naturalistic method* and the *experimental method*. The first includes all careful, critical, systematic attempts to observe the behavior of animals under wholly natural conditions—the study of life-habits, instinctive adjustments, social relations, memory. The second includes all the diverse ways of observing animals under artificially controlled conditions. These two methods of study should supplement one another. To divorce them means to retard our science. The naturalistic method should yield sympathetic familiarity and intimacy with the ways of animals; the experimental method should furnish accurately describable and verifiable facts whose interpretation is rendered possible by naturalistic knowledge. I believe I am not overemphasizing the facts when I say that every experimentalist in animal psychology should be also a naturalist whose love of animals and sympathy with them enables him to understand their behavior. Nor does it seem to me less important for the progress of our science that the naturalist should be also an experimentalist. For only thus can he hope to bring to prompt and decisive solution those problems of behavior which his naturalistic observation constantly suggests. We need naturalists and we need experimentalists, and, above all, the welfare of Animal Psychology demands their sympathetic and intelligent coöperation. If those who decry experimentation are the misguided enemies of science, surely those who, instead, ridicule observation under natural conditions are its narrow-minded servants."

These paragraphs are in point chiefly because each new generation revives the misunderstandings of naturalists and experimentalists. Is it chiefly because of temperament, experience, or accident that we tend to exalt the one or the other type of method and its results and are too often inclined to belittle the other?

(*2*) *Breeding and observation station.* The provisions for anthropoid research suggested by the phrase "breeding and observation station" are a step removed from the conditions of naturalistic field work, and are intended to give a larger measure of control over subject and conditions of observation. The purpose of such an establishment and the work therein is mainly to provide intimate knowledge of the entire life-history of the anthropoid ape, and thus to render available, for special use in problems which demand such information, individuals of known ancestry, age, experience, and conditions of development. Except as a subject's life-history is intimately and reliably known, it is impossible to proceed satisfactorily in the study of many of the types of psycho-biological problem enumerated in the foregoing program. A breeding and observation station is therefore an absolute essential for the supplementation of field investigations on the one hand, and of work which may be done in special biological laboratories on the other hand.

(*3*) *Special laboratories.* Obviously certain lines of work must be conducted in adequately equipped special research establishments rather than in field or breeding station. This applies as well to various lines of psycho-biological inquiry as to the morphological and physiological. Consequently the coöperation of varied special biological laboratories is essential to the ideal execution of the program. For specialization on a variety of psycho-biological problems, obviously there should be at least one laboratory, and preferably more because of the diversity of type in the anthropoid apes, especially equipped and manned for the purpose, as is the Primate Laboratory of the Yale Institute of Psychology.

The general question, how proceed with this program, I have answered by suggesting three types of observational situation, each with its peculiarly appropriate opportunities and techniques. Each may be considered for present purposes an anthropoid laboratory, and each should yield uniquely valuable results. Thus the three types of establishment should tend to supplement one another's resources and findings.

## V. Summary

Answers to the questions why, what, how, in anthropoid research, may thus be summarized.

*Why?* Because of their relatively greater availability, despite numerical deficiency, controllability, psycho-biological simplicity,

resemblance to man, and the peculiar values of results obtainable, it is maintained that the anthropoid apes should be used as subjects of various genetical, psychological, sociological, pedagogical, and hygienical inquiries.

*What?* The program of anthropoid research proposed in reply to the question what to do, although primarily psycho-biological, recognizes the essential contributory values of ecological, morphological, and physiological techniques, facts, and principles, and assumes their availability and utilization through the coöperation of competent specialists.

By enumeration of thirteen major assemblages of problems or fields of inquiry, each of which is briefly characterized and exemplified by selected problems, a program of anthropoid research has been schematically presented. The grouping of problems is practical rather than logical and the list which follows makes no pretense at completeness. (1) Action system; (2) receptivity and sensibility; (3) expressivity and affectivity; (4) temporal relations of psycho-biological phenomena; (5) perceptions, configurations, coördinations; (6) mnemonic processes; (7) ideation, insight, thought, generalization; (8) motivation; (9) adaptivity and education; (10) language and symbolism; (11) norms and differences: individual, sex, age, species, environmental; (12) social relations; and (13) conduct and morality.[9]

*How?* For the furtherance of this program, work on parts of which is already in progress, the systematic, continuous, extensive and intensive use of three major types of observational situation and of appropriate techniques in each is proposed. They are: (1) field or natural habitat; (2) breeding and observation station; and (3) special laboratories.

The first should provide for either naturalistic or experimental study of subjects under the natural conditions of freedom, and eventually should yield reasonably complete and reliable general description of the psycho-biological characteristics, ecological relations, and daily life of individual and species.

The second type of establishment is necessary to provide suitable opportunity for continuous and intensive observation of life-history,

[9]At present the investigators of the Yale Primate Laboratory—Mrs. Lewis, Messrs. Bingham, Tinklepaugh, Upton, and Yerkes—are engaged on specific problems, the development of techniques, or the assembling of observational data which represent all of these categories, and in addition, "physical measurement, growth, and nutrition."

development, and experience, so that genetic description may be extended, and individuals of known ancestry, age, experience, and psycho-physiological condition made available as subjects for attack on specific problems in special biological laboratories or as breeding specimens. Under existing conditions, psycho-biological research is seriously hampered and its results compromised by the necessity of utilizing subjects of unknown origin, age, and history. The situation is inexcusably wasteful and discouraging.

The third represents the final step toward specialization of resources for the advancement of the program. By special laboratory is meant a research establishment which specializes on a particular division of biological inquiry and in which anthropoid material may regularly or on occasion be advantageously used. The Primate Laboratory of the Yale Institute of Psychology especially provides in its technical equipment and personnel for certain phases of psycho-biological study of anthropoid apes. It is specialized in at least two directions, for ordinarily attention will be limited to psycho-biological problems, and for their solution primate subjects will be used.

# COMPARATIVE PSYCHOLOGY AND PSYCHOPATHOLOGY

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## I

Investigations within the general fields of comparative morphology and comparative physiology have been of vital importance in the development of the various specialties of scientific medicine for reasons which are easily made apparent. In the first place, with human material the welfare of the individual must always take precedence over the methodological requirements of any research which aims at the collection of scientifically interpretable data, whilst animals may be dealt with in any way which does not involve cruelty. Again, simpler prototypes of relatively complex manifestations of human life can usually be found at some level in the phyletic scale below man, thus affording valuable orientations as to the human phenomena with which the medical investigator is dealing. Finally, the animal subject is, as a rule, much more amenable to procedures calling for controlled observation than is the human subject. It is a truism of medicine that our knowledge of (1) how the human organism is constructed and of its structural ontogenesis, (2) how it functions in health and disease, and (3) how it responds to a great variety of prophylactic and therapeutic measures is almost as important a derivative of experimental and other observations of animals as it is of our direct dealings with human clinical material. All this is so well known, and has been said so many times, that its repetition here would be unwarranted if it were not that the value of the comparative approach has been largely overlooked in the very field of medicine in which there is greatest need of facts of animal life for purposes of correlation with our own findings. I refer, of course, to psychopathology, which deals with phenomena of even greater complexity than, for example, those of human metabolism or of endocrine function. An effort will be made in what follows to point out a few of the many problems of psychopathology which cannot be adequately investigated without the assistance that could be had from correlated studies of animal behavior.

The problem of making scientifically interpretable observations of human behavior is always, in itself, a difficult one, and in psycho-

pathology it is enormously complicated by the nature of the patients' reports of their attending states of consciousness. To many psychopathologists this difficulty appeared to be an insurmountable one after Freud's theories had reached a sufficient degree of development and of support in admitted facts of clinical observation to be taken seriously. He has brought us to see with ever-increasing clearness that overt human behavior and the individual's reported attending states of consciousness (to which I will hereafter refer as "overt psychical reaction") often have *for their primary endogenous determinants* adjustive impulses which do not come to direct expression in overt behavior and physical reaction. In other words, what the individual (1) does about a given situation, (2) how he feels about it (as sincerely reported to us by him), and (3) what he believes to be his intention with regard to it may be indirect and highly distorted expressions of primary adjustive impulses which cannot come to direct expression in overt response because of interfering inhibitions. Thus the son who (1) frantically summons a physician to the bedside of his stricken father, (2) experiences poignant grief at the prospect of a fatal termination, and (3) sincerely proclaims his intention to make every necessary sacrifice in his father's behalf, may have been so conditioned throughout childhood and adolescence that the adjustive impulsion primarily touched off by the father-illness situation would find its most direct expression in (1) efforts to block the summoning of a physician, (2) a feeling of gratification that an ancient rival may be quitting the scene, and (3) an intention to further his father's demise. Of course the situation would be much less simple than this in real life. Ambivalency phenomena, for example, would enter into account here, but the illustration will serve our purposes at this point without unfairness to Freud.

Now the psychoanalytic technique aims at making observations which will disclose not only the nature of the primary adjustive impulsion immediately behind such indirect modes of overt behavior and psychical reaction, but the earlier determinants of a reactive set (Oedipus complex in this case) which makes the rise of such an impulsion possible and its habitual inhibition from direct overt expression predictable. An examination of this technique is likely at first to discourage the psychologist who has been trained to respect certain rules of evidence to which science is committed, and to leave him with the impression that the disclosure of the really significant determinants of such reactions calls for the exercise of a subtle art of

clinical examination which, however valid it may be in its own right, is in no sense of the word a scientific procedure. Among psychoanalysts themselves there is a tendency either to liberalize the meaning of the term 'scientific method' to a point at which it loses all meaning or to abjure 'science' as an obsolete hobble which is not to be tolerated by non-sterile investigators. Such views or attitudes as these fail to take into account the history of the origin of most technologies—particularly of those which we have in mind when we use the collective term 'Medicine.'

Deliberate efforts to develop an orderly, scientifically grounded technology for the control of natural phenomena are usually preceded by a good deal of empirical testing of possible methods of control which are suggested by impressions derived from non-scientific observations. This has been particularly true of agriculture and medicine, in which fields practical endeavor cannot always await the guidance of scientifically formulated principles. With the introduction of fragments of scientific methods and principles there comes a period during which these are pieced out with impressions derived from practical experience. The result is that adequately verified findings and purely impressionistic guesses are indiscriminately integrated into a quasi-technological system. General medicine was at the peak of this stage of its development when Osler, among others, paved the way for truly technological developments by demanding that there be no further confusion of the findings of clinical impressionism with those of critically elaborated methods of investigation. I was about to say that psychiatry is the technology which has psychopathology for its specific and most proximate theoretical foundation when I realized that I also wish to say that psychiatry is still in the quasi-technological stage in which Osler found general medicine. This will be apparent, perhaps, when we examine Freud's psychoanalytic technique with a view to estimating its adequacy for purposes of research in scientific psychopathology. When the psychoanalyst studies an individual whose habitual modes of response to particular types of situations are more or less seriously maladaptive, the procedure is somewhat as follows. Once the preliminary interviews are over the patient is given a series of sessions of one hour each for four, five, or six days a week over a period of from six months to two years. The analyst who follows Freud closely has the patient lie upon a couch or sofa and takes his own position in a chair behind and out of sight of the patient. Freud's instructions to the patient

at this stage seem to me to be much sounder from a methodological point of view than are any of the variations of his technique that are followed by other analysts. A direct quotation from one of his articles on technique will serve our purpose here.[1]

> What subject-matter the treatment begins with is, on the whole, immaterial, whether with the patient's life-story, with a history of the illness or with recollections of childhood; but in any case the patient must be left to talk, and the choice of subject left to him. One says to him, therefore, 'Before I can say anything to you, I must know a great deal about you; please tell me about yourself.'
>
> The only exception to this concerns the fundamental rule of the psychoanalytic technique which the patient must observe. This must be imparted to him at the very beginning: "One thing before you begin. Your talk with me must differ in one respect from an ordinary conversation. Whereas usually you rightly try to keep the threads of your story together and to exclude all intruding associations and side-issues, so as not wander too far from the point, here you must proceed differently. You will notice that as you relate things various ideas will occur to you which you feel inclined to put aside with certain criticisms and objections. You will be tempted to say to yourself, 'This or that has no connection here, or it is quite unimportant, or it is nonsensical, so it cannot be necessary to mention it.' Never give in to these objections but mention it even if you feel a disinclination against it, or indeed just because of this. Later on you will perceive and understand this injunction, which is really the only one that you have to follow. So say whatever goes through your mind. Act as if you were sitting at the window of a railroad train and describing to someone behind you the changing views you see outside. Finally, never forget that you promised absolute honesty and never leave anything unsaid if for any reason it is unpleasant to say it."

It is the analyst's function to "unfold to (the patient) the hidden meaning of his thoughts and associations, to initiate him into the postulates of analysis and its technical devices."

I am quoting Freud out of context in this last paragraph, which is taken from a page devoted to stressing the importance of deferring any such contributions from analyst to patient until "a dependable transference, a well-developed rapport, is established in the patient."

This necessarily much abridged and over-simple account of psychoanalytic technique has been given here in order to lay a foundation for disclosing to the comparative psychologist the nature of some of the problems with which we have to deal in research psychopathology. It is one thing to obtain the relatively 'free,' spontaneous, and uninhibited productions of a subject who has been induced to lie on a couch and report what is passing through his

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[1] S. Freud, *Collected Papers*, 1924, vol. 2, 355-360.

mind, and quite another matter to interpret them in terms of what adjustive impulsions are primarily behind the subject's overt or symptomatic behavior and mentation. Inspired guesses may, and often do, serve the purposes of the therapeutist, but somewhat more convincing inferences must be made possible if psychiatry is ever to find direction in the findings and principles of a scientifically developed psychopathology. As I see the problem we must first ask ourselves two highly important questions.

(*1*) To what extent are the subject's spontaneous productions determined by his previous knowledge of psychoanalytic theory and by the analyst's procedure in currently "unfolding to him the hidden meanings of his thoughts and associations" and "initiating him into the postulates of analysis and its technical devices"?

I should like to know, for example, what I should have produced during my first ten sessions if I had been naïve as to psychoanalytic theory at the inception of my analysis, and if I had been kept naïve in this respect throughout the period of those sessions. It is also a matter of considerable interest to me when I am playing the rôle of analyst to speculate as to how much of valid material and how much of artifact one may expect from a patient who starts his analysis with at least a smattering of psychoanalytic theory and who currently picks up from me an undetermined amount of sophistication.

(*2*) Assuming the possibility of obtaining subjects who are naïve as to psychoanalytic theory and of keeping them in this state of unsophistication until the end of a series of fifty or a hundred sessions, how are we going to proceed in developing satisfactory criteria for the interpretation of what these subjects produce? Freud has certainly supplied us with none which would be acceptable in a court of law, a court of science or in any other court in which obviously valid and binding rules of evidence are respected.[2]

The first of these two questions must, of course, be formulated as a problem for research psychopathology; but comparative psychology has something of value to contribute even here for our orientation. As I have elsewhere shown,[3] man is not the only animal whose adjustive impulsions may be inhibited from coming to direct overt expression, only to push through to various modes of indirect expression.

[2] I may say, parenthetically, that I am very glad that Freud never subjected himself to such hobbling, since otherwise we might never have had the benefit of his amazing genuis for arriving at a series of 'inspired guesses' which the empiricist finds, on the whole, so convincing.

[3] G. V. Hamilton, *Introduction to Objective Psychopathology*, 1925.

My work with monkeys has shown that such phenomena can be experimentally elicited in the behavior of these animals. A recent visit to Yerkes' summer anthropoid station profoundly impressed me with the importance of making known to the comparative psychologist the need of psychopathology for information about the anthropoid counterparts of human modes of adaptation to environmental conditions which induce inhibition and consequent indirect overt expression of various primary adjustive impulsions. His work with chimpanzees suggests that these animals are peculiarly well suited for such purposes.

The ideal procedure to follow when one seeks to explain a given set of phenomena is, in principle, the same whether one is studying a rat, a chimpanzee, or a psychoneurotic patient. It calls for more than merely a shrewd and detailed observation of the phenomena to be explained; if the method of observation is in itself a factor in determining the occurrence of what is observed this fact must be given due consideration. The present technique of psychoanalysis is such that one must have the spirit of the advocate rather than that of the scientist to be blind to the fact that we have no way of knowing what part of the material produced by a patient under analysis is, as it were, an artifact produced by his sophistication. This difficulty is increased, if anything, for the student of research psychopathology when he is convinced by his own experience as a clinical psychiatrist that a great deal of such material is actually a more or less direct expression of vagrant adjustive impulsions. The methodological problem here is a two-fold one, viz., (1) to obtain unsophisticated subjects and to keep them so throughout a sufficient number of sessions, and (2) to effect this without thereby sacrificing the possibility of developing the "dependable transference" to which Freud alludes in the above quotation. This, as I have said, is essentially a problem for research psychopathology, to the solution of which comparative psychology can indirectly contribute by making supplementary studies of animal behavior.

## II

Most psychoanalysts would agree, I believe, that if research objectives were given unqualified precedence over the therapeutic urgencies of a given case it would be quite possible to obtain the kind of material upon which Freud has based his main theories and at the same time to meet the methodological requirements that are outlined above. Once such material is obtained there will come the far more

difficult task of interpreting it. The therapeutist, including Freud himself, follows the method of clinical impressionism in inferring from the patient's productions what these signify as to underlying adjustive impulsions, conflicts of such impulsions, their earlier determination and the nature and ontogenesis of the inhibitive habits which stand in the way of their direct expression in behavior and mentation. I do not wish to belittle the therapeutic value of such inferences when they are made by men of great shrewdness, penetrating insight and experience, but they are at best impressionistic guesses. It is the best that the analyst can do at present, and in competent hands it is so much better for therapeutic purposes than the coddling and vague preachments of most non-Freudian psychiatrists that I am likely to be rated as an obstructive misoneist when I say that it is still very remote from the kind of scientific realism which has given us scientific medicine. When an analyst says to his patient, "What you have produced during the last several sessions discloses the operation of a dynamically important Oedipus (of homosexual or what-not) trend, and to this I attribute your major symptoms," the patient might justly retort in most instances, "Yes, possibly so; but how do you know? Other, equally valid inferences might be made from my recent productions." Such an objection on the patient's part would, of course, be interpreted by the analyst as evidence of resistance to a now still further substantiated inference. If the analyst were required to defend his inferences in a less arbitrary manner he would find no body of correlated scientific findings to fall back upon.

Those of us who are familiar with research procedure in both medicine and comparative psychology know that it is often necessary to go far afield in quest of supplementary facts before we can so formulate our methods of observation that our findings will carry with them the story of their own determination. The comparative psychologist, for example, is able to obtain findings which we at once accept as disclosing that a given animal has or has not exhibited ideational behavior under the conditions of his experiment, not because these findings carry in themselves sufficient grounds for such an inference, but because this inference is justified in light of a host of preceding scientifically valid observations in such diverse fields as the physiology of the sense organs, micro-organic behavior, and experimental neurology. Similarly, the cardiologist finds his diagnoses of heart conditions impressively revealed by electro-cardiographic and clinical records only because this is evident in light of facts elicited by

many generations of investigators in various fields. In psychopathology, even more than in medicine and comparative psychology, our direct findings do not carry with them *prima facie* evidence as to what factors have entered into their determination.

There is, of course, anthropology for the psychopathologist to fall back upon for supplementary findings in light of which he may make his inference from his own material. There is an extensive literature devoted to accounts of the myths, legends, customs, taboos, and tuitional impositions of primitive peoples, all of which has a suggestive value. But anthropology is not sure enough of its own methods in this field to give to its findings more than a suggestive value for purposes of correlation with the findings of psychopathology. Paleopsychology may be dismissed without further consideration, as being a largely speculative discipline which is much more a recipient than a donor in its relation to psycho-analysis. Child psychology and the scientific study of animal behavior, on the other hand, have much of value to offer psychopathology, and investigators in both these fields are in a position to contribute a great deal more, once we psychopathologists form a habit of acquainting them with our specific needs.

Of course child psychology labors under the disadvantage that is inherent in all fields of investigation which deal with human material. Many of its findings, like those of psychopathology, can be obtained only under conditions which impair their validity for purposes of interpretation. Nevertheless, we psychopathologists are vitally interested in what the child does and says, because we wish to know as much as possible about the origin of its reactive tendencies, the dynamic components of its reactive equipment and the modifiabilities of this equipment. The more the student of child psychology has to tell us about all this the better shall we be equipped to shape up our own methods of observation and to interpret what we find. The student of child psychology has, in turn, a similar need of information about the reactive tendencies, reactive equipment, and reactive modifiabilities of animals. Such information ought, ideally, to cover the entire phyletic range, but the most urgent present need of this kind is for information about the anthropoid apes.

## III

During the past two years I have been engaged in a research in which I have sought to secure comparable data by a method which cannot be described within the limitations of the present discussion,

but to which some reference must be made here in order to explain upon what basis of concrete findings the following proposals for research in the field of animal behavior are offered to comparative psychologists for their consideration.

Two hundred educated spouses of the younger generation—one hundred of each sex—were studied with reference to two main research objectives: (1) to obtain as accurate descriptions as possible of the individual subjects' different present modes of spousal adjustment; and (2) to elicit by psychiatric examination detailed information about the various conditioning environmental factors and specific personal experiences to which one might attribute the determination of present spousal reactive tendencies. Throughout the twenty-one months devoted to the examination of these two hundred spouses and since then, during the analysis of their records that is still in progress, I have found myself formulating problems for research in animal behavior in response to my own specific needs for a better orientation as to the human phenomena with which I was dealing. These problems, as they are listed below, will serve to exemplify the points that I have sought to make in the foregoing discussion.

(*1*) A and B, two human individuals of the same age, sex, and nationality and with similar cultural backgrounds encountered while together a situation which appears to have so conditioned A that directly thereafter and during the remainder of her life up to the period of her examination a given type of stimulation had for her a negative (and therefore maladaptive) reactive value. The same type of stimulation continued to have for B the positive (or normal) reactive value that it previously had for both girls.

There is need of experimental studies of animals of a given species, age, sex, etc., for the demonstration of how and why it is possible for one or more members of such a group to become critically conditioned to exhibit a new and maladaptive habit of response to a specific type of stimulation whereas other members of the group remain unaffected by the conditioning situation.

(*2*) In psychiatry we often seek to modify the reactive equipment of an individual in such a way that a given type of situation which has for him an undesirable reactive value will acquire a very dissimilar or even opposite reactive value. At present success in such undertakings appears to be a function of native shrewdness and empirically acquired 'knack' rather than of an effective grasp of clearly understood technological principles.

Appropriate experimentation with animals might disclose both general and specific principles to follow in seeking to modify the reactive values of particular types of stimuli.

(*3*) A significantly large percentage of women—perhaps as many as 50%—are distinctly subnormal as to capacity for experiencing the normally terminative climax of the physical marital act. The majority of these subnormal cases that have come under my observation have made full intellectual correction of interfering beliefs and attitudes acquired during childhood and adolescence, and the intensity of their sex desire appears to be at least as intense as is that of women who are relatively normal as to capacity for experiencing the terminative climax. Furthermore, the subnormal ones give evidence of fertility equal to that of the normal ones. Since in women capacity for experiencing the climax is not essential to reproduction it has been argued that the persistent absence of such a capacity does not necessarily argue the operation of post-natal negatively conditioning factors. This contention assumes that since the climax has no adaptive value for purposes of reproduction it is subject to the operation of a so-called principle which accounts for marked variability (as to presence or absence) of all non-adaptive functions.

Two important problems are apparent here:

(*a*) Wherever the sex act occurs among animals below the primate level it has direct 'value' for the continuation of the species. Its restriction among them to definite physiological periods when conception is almost an inevitable result in the healthy female suggests that it has for them only a single adaptive value, viz., that which is directly related to reproduction. But among the infrahuman primates that I have been able to study, the free, healthy animal of both sexes seeks and engages in the sex act at all times after maturity is reached except that the female does not do so during the last few days of pregnancy and for a few weeks after parturition. This is likewise true of the human primate except where aesthetic considerations or specific taboos interfere. Has the sex act an adaptive value for primates in addition to and distinct from that which relates to reproduction? Careful observations of monkeys and apes under favorable conditions ought to throw a great deal of light upon this important problem.

(*b*) Consideration ought to be given to the possibility of modifying the sex behavior of monkeys and apes by subjecting them during infancy and adolescence to the counterparts of both the planned and unplanned influences to which our human children and adolescents are commonly subjected.

(*4*) Both my previous clinical studies and my present research suggest that there are individuals who have been so conditioned during childhood or adolescence that all beloved persons of the opposite sex have for them the negative sexual value that close blood relatives

of the opposite sex have for most of us. There are at least two problems here upon which appropriate studies of animal behavior may be expected to throw a much needed light.

(*a*) Casual observation suggests that the male infra-human primate can be so conditioned that he will not engage in sex plays with a particular unrelated female of his group, even though (or, perhaps, because) he habitually displays a greater preference for her companionship than for that of any of the females with whom he engages in sex plays. There is need here for extensive and carefully planned observations.

(*b*) Do any mammalian species give evidence of an innately determined 'incest barrier,' or is avoidance of incestuous relations always a resultant of post-natal conditioning?

(*5*) Wherever the family structure is similar to that of our occidental culture the young male child is apt to pass through a period during which he manifests jealousy of the father for the mother's affections, and to say or do that which suggests hostility toward the father and a desire to eliminate him.[4] Many cases of serious psychoneurosis in adult life clearly manifest symptomatic indirect reactions to a held-over underlying impulsion to react to the father or his surrogates as to a rival and an enemy. The primary adjustive impulsion involved here often comes to strikingly direct overt expression in the behavior of the little boy, and it is as yet by no means clear that its lapse implies in every case the rise of inhibitions which will thereafter permit only indirect overt expression. The little boy's tendency to be jealous of the father and to exhibit hostility toward him may continue as an inhibited but dynamically important component of the reactive equipment in only those cases that have been subjected to preventable environmental influences.

The Oedipus complex of Freud's description is worthy of examination from every possible angle. The fact that among mammalians there are many species in which the male characteristically assists in rearing the young offers an opportunity for much valuable research in this field. Do the young of such species pass through a period of antagonism to the male parent? What is their reactive relationship to the male parent after they have reached maturity? Is it like or unlike that which is manifested toward other males of the father's generation? Are the reactions of the female offspring to the male parent like or

[4] Bronislaw Malinowsky states in a forthcoming book that the Oedipus complex of Freud's discription does not occur among people whose family structure is of the matrilineal type. It is to be hoped that anthropologists with Malinowsky's grasp of psychopathological problems will be stimulated by his work to make further investigations along this line.

unlike those of the male offspring? These are only a few of the many Oedipus problems that could be turned over to the comparative psychologist for his consideration.

A volume might be devoted to the mere listing of such problems as are given above. For example, the so-called polymorphous perverse tendencies of childhood to which Freud alludes call not only for critical and unbiased studies of childhood but for a quest of their prototypes in the behavior of animals; but I have limited my list of proposals for research in animal behavior to problems which have been especially brought to the foreground by my present research in the problems of marriage. It is, of course, obvious that both comparative psychology and psychopathology would be greatly benefited by more extensive correlations of their respective findings and more specifically purposeful coördination of research effort. This will come about as a natural development if two general sets of conditions can be made to obtain.

(*1*) Teachers and investigators in the field of comparative psychology must take greater pains to acquaint themselves with the currently arising problems, methods, concepts, research activities and findings of psychopathology.

(*2*) We psychopathologists must begin by being fearlessly iconoclastic with reference to our own impressionistic guesses, and then formulate our problems as concretely and as objectively as possible so that we may invite supplementary research in other fields.

# THE LAW OF EFFECT

By EDWARD L. THORNDIKE, Teachers College, Columbia University

The alleged law of effect, that what comes after a connection acts upon it to alter its strength, has suffered disfavor from psychologists and neglect from physiologists. It is, compared with apparently more mechanical forces like frequency and intensity, indefinite and troublesome to think with. It has been even more odious to philosophers and educational theorists who find it a dangerous antagonist to, or an inferior substitute for, their explanations of behavior by purposes, relief from tensions, and other teleological or quasi-teleological factors.

It was invented as a purely empirical hypothesis to explain the facts of learning or modifiability in animals which frequency could not explain. It was extended as a fundamental law to assist in explaining the general potency of wants, interests, purposes and desires in education and elsewhere. The chief justification of it in the past has been the fact that when 'annoyingness' is attached to a frequent connection and 'satisfyingness' to a rare connection, the latter gains and the former loses until the latter becomes the habitual response. Recent experiments[1] have shown that the frequent will not, by its frequency alone, gain at the expense of the rare, so that when a frequent response grows more and more frequent, some law of effect is needed as truly as when the rare displaces the frequent. So now, more than ever, the law of effect deserves a more thorough-going investigation.

I report here what I hope may be one step in such a thorough investigation. The experiments to be reported here were designed to be, and I think are, a crucial test of whether the after-effects of a connection do in fact strengthen or weaken it.

The effects which have practically monopolized what little discussion there has been are satisfyingness and annoyingness, or states of affairs considered according to the amount of satisfyingness or annoyingness which they possess. It would probably be profitable to study also excitement and calm, sensory pains and sensory pleasures,

[1] A preliminary report of these was made in *Proc. Nat. Acad. Sci.*, Jan., 1927.

muscular tension and muscular relaxation, restlessness and quiescence, and other effects of the action of connections; but we shall limit ourselves to the effects to which primary importance has been attached by the adherents of the doctrine.

The actual effects used in the experiments were the words "right" and "wrong" spoken by the experimenter about 2 sec. after the *S*'s response.

### Experiment A

A series of 50 strips of paper, two of every unit length between 3 and 27 cm., and alike in every respect except length, was presented on a fixed background in a random order. The *S*, who had before him a strip 10 cm. long and known by him to be 10, estimated the length of each strip in integral numbers. The 10-standard was kept fixed in a spot to the right of the lengths to be judged. The *S* knew nothing concerning the constitution of the series of strips, save that they were all integral multiples of one-tenth of the standard. He never saw the strips except one at a time during the experiment.

After 50 judgments had been made with no aid save that which the *S* could derive from the standard, and with no statement to him of any sort about the results, the series was presented in the following manner. A strip was placed before *S*. As soon as *S* announced his estimate, the strip was withdrawn, placed behind a screen and turned over; and the experimenter said "right" or "wrong," according as the estimate was right or wrong. No statement was made of the amount or direction of the error. The experimenter then recorded the estimate, and presented another strip. The statement of "right" or "wrong" came approximately 2 sec. after *S*'s announcement of his estimate, and approximately $1\frac{1}{4}$ secs. after the strip was removed from his sight. After a number of such presentations of the 50 strips (usually seven, two or three a day for three or four days), the series was presented with no aid, save the presence of the standard 10, as in the first trial.

The results of these experiments are like those where animals are rewarded by food or freedom or the like, after certain connections act; and are punished, or at least not rewarded, after certain other connections act. The satisfying connections are strengthened and the curve of successes rises indubitably and rapidly. The results are in striking contrast to those of experiments of similar nature in which no "right" and "wrong" consequences are attached to the connections.

Table I shows the essential facts for 5 *Ss* in the case of whom the connections produced the effects of "right" and "wrong." All *Ss* improved greatly. The average percent of reduction in the error was 61 with a probable error of ±4.

TABLE I

THE INFLUENCE OF EFFECT: The accuracy of estimates of lengths 3 cm. to 27 cm., a 10-cm. line being shown.

| *S* | Test before training | Sums of deviations from the true lengths; training with "right" and "wrong" | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Training periods | | | | | | | | Test after training | Percent of reduction |
| | | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | | |
| A | 33 | 47 | 32 | 31 | 27 | 22 | 22 | 13 | 14* | 12 | 64 |
| Le | 68 | 59 | 46 | 49 | 43 | 37 | 26 | 26 | 41 | 35 | 49 |
| Li | 70 | 47 | 19 | 29 | 25 | 27 | 25 | 25 | | 20 | 71 |
| S | 112 | 117 | 106 | 96 | 61 | 49 | 30 | 34 | | 26 | 77 |
| Wi | 36 | 28 | 38 | 24 | 25 | 13 | 29 | 16 | | 20 | 44 |
| Sum | 319 | 298 | 241 | 229 | 181 | 148 | 132 | 114 | | 113 | |

Average, 61 ± 4 Median, 64

*Estimate from 25 judgments.

TABLE II

THE INFLUENCE OF MERE REPETITION: The accuracy of estimates of lengths 3 cm. to 27 cm., a 10-cm. line being shown.

| *S* | Test before training | Sums of deviations from the true lengths; training with mere repetition | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | Training periods | | | | | | | Test after training | Percent of reduction |
| | | 1 | 2 | 3 | 4 | 5 | 6 | 7 | | |
| T | 34 | 21 | 27 | 24 | 24 | 18 | 37 | 20 | 36 | −6 |
| K | 61 | 73 | 41 | 50 | 55 | 48 | 60 | 58 | 40 | 34 |
| R | 50 | 38 | 52 | 42 | 38 | 40 | 37 | 33 | 49 | 2 |
| H | 54 | 38 | 48 | 34 | 33 | 25 | 52 | 35 | 35 | 35 |
| M | 99 | 126 | 125 | 128 | 126 | 89 | 79 | 93 | 89 | 10 |
| C | 91 | 70 | 84 | 107 | 66 | 85 | 77 | 93 | 116 | −27 |
| F | 43 | 55 | 70 | 53 | 54 | 55 | 78 | 72 | 86 | −100 |
| Sum | 432 | 421 | 447 | 438 | 396 | 360 | 420 | 404 | 451 | |

Average, −7 Median, 2

The gain came partly by a reduction in constant errors for Le, Li, and S, and partly by a reduction in the variable error for all. In the test before training Le had a total of 52 units too low and 16 too high; Li judged the lengths 15 or below too low in 19 cases out of 26, and judged the lengths 16 or above too high in 19 cases out of 24; S had a total of 111 units too low and 1 too high. In the final test Le's constant error was such as to produce 23 minus and 12 plus errors; Li's had been reduced to a slight tendency to estimate all lengths too low; S's had changed to a slight error in the opposite direction.

The reduction in the variable error may be measured by the differences between the two early estimates and the differences between the two later estimates for the same lengths. The sums of these differences were:

| *S* | Before Training | After Training |
|---|---|---|
| A | 19 | 8 |
| Le | 36 | 21 |
| Li | 37 | 10 |
| S | 30 | 12 |
| Wi | 25 | 12 |

With Table I we may contrast Table II, which presents similar measurements in the case of subjects of similar general intelligence and initial ability in estimating the lengths in the case of whom the same number of experiences occurred, but with no effects in the shape of announcements of "right" and "wrong."

As a general check on the universality of the facts, and as a special check on the remote possibility that some personal influence of the experimenter might have in some way distorted the facts, Mr. Abelson did me the favor of repeating the experiment with 6 *Ss*, but with five instead of seven presentations with "right" and "wrong" as effects. The control *Ss* similarly had five presentations with no aid. The results, shown in Table III, are closely similar to those of the first experiment. The average percent of reduction in error is 50, with a probable error of ±5. The average percent of reduction for the control group is 6 with a probable error of ±12.

These experiments are crucial as a demonstration that the consequences of a connection work back upon it to influence it. There is no difference between the 'aided' and 'no aid' experiences save in the consequences of the connections. It is also evident that the

consequences probably act on the connection directly, and not by leading the *S* to repeat it, or something like it, to himself. Strictly speaking, he could not repeat *it*, but at most could hold in mind some sort of image or illusion of the strip and repeat the approved estimate as a response to this image or illusion. He had little time to do this because a new length was presented about a second after the "right" or "wrong" announcement was made. It is improbable that such images would be sufficiently accurate to serve as substitutes for the real strips. We hope later to carry out special experiments to settle this question.

TABLE III

THE INFLUENCE OF EFFECT: The accuracy of estimates of lengths, 3 cm. to 27 cm., a 10-cm. line being shown. Data by Abelson. Sums of deviations from the true lengths

| Tests before and after 5 periods of training with "right" and "wrong" | | | | Tests before and after 5 periods of training by mere repetition | | | |
|---|---|---|---|---|---|---|---|
| *S* | Before | After | Percent of reduction | *S* | Before | After | Percent of reduction |
| A | 53 | 37 | 32 | G | 70 | 61 | 13 |
| B | 72 | 13 | 82 | H | 56 | 104 | −86 |
| C | 57 | 40 | 30 | I | 60 | 54 | 10 |
| D | 34 | 14 | 59 | J | 66 | 46 | 30 |
| E | 106 | 67 | 37 | K | 82 | 59 | 28 |
| F | 128 | 49 | 62 | L | 136 | 80 | 41 |
| Sum | 450 | 220 | | | 470 | 404 | |
| Average | | | 50±5 | Average | | | 6±12 |
| Median | | | 48 | Median | | | 21 |

It is of course true that a continued success with some one strip, say 3, may influence the estimates of other strips. The 25 strips form to some extent one experiment, not 25 absolutely independent experiments. Being sure that you would know that a short-looking strip was 3 if it really was 3, and thinking that it is not 3, you are profitably led to call it 4. Such facts do not, however, deny that fundamentally the consequences act directly on the connections. And quantitatively, these indirect secondary aids are of relatively little consequence. They often teach the *S*s that 3 is probably the smallest strip that will be presented to them, but 2, 3, and 4 are easily learned, apart from this probability. No *S* gained any surety about the probable upper limit of the series. The greatest aid from this in-

direct ratiocinative use of the consequences seems to be gained when a *S*, feeling fairly sure that certain strips are, say, either 13 or 14, either 16 or 17, either 20 or 21, observes that his lower (or higher) estimates are right and thereafter deliberately favors the lower (or higher) estimate in such cases. Such ratiocinations may, however, be prepared for and based on direct strengthening of certain connections by the "right" and "wrong" consequences. They are also themselves in part constituted by the attachment of "right" to the connection: "My lower estimates are better than my higher." And they do not account for the reduction of variable errors.

## Experiment B

In experiment B,[2] the connections were between the commands, "Draw a 3-inch line," "Draw a 4-inch line," "Draw a 5-inch line," "Draw a 6-inch line," and the response of drawing under conditions described below. The effects were "right," said by the *E* when the line drawn was within ⅛ in. of the correct length in the case of 3-in. lines, or ¼ in. of the correct length in the case of the other 3 lengths, and "wrong" said in other cases.

The procedure in learning to draw lines of stated lengths with eyes closed was as follows:

The *S* was seated, blindfolded, at a table opposite the *E*, and in front of a drawing-board, along the left-hand edge of which a strip of veneer, about 2 in. wide, had been fastened in such a way that a large sheet of cross-section paper (16 x 21 in.) could be slipped between it and the board and fastened to the board by means of two or three carpet tacks. The strip of veneer served as a fixed starting edge for all lines. The cross-section paper itself was so ruled, in pencil, as to make it possible for the experimenter to tell readily the length of any line drawn from the strip as a zero point.

The *S* was instructed to draw lines of a given length, starting always from the strip of veneer at the left, and to wait after each line until hearing the score called, before drawing the next line. He was required to draw each line with one continuous, quick movement.

The *S*s were trained to draw 3-, 4-, 5-, and 6-inch lines at a single sitting, and were instructed to draw lines of a given length, waiting

[2]The writer is responsible for the general plan of experiment B and for the treatment of the results, but the credit for the details of its execution belongs to Dr. Elsie O. Bregman. With the assistance of members of the staff of the Institute of Educational Research, she conducted all the tests and training and supervised the scoring and tabulating of results.

after each line to hear its score, until directed to draw lines of a different length. The number of successive repetitions of a single length varied between 4 and 8, and the lengths followed each other, not numerically, but in a random order, according to a prearranged scheme. This scheme, which we shall call the 600 series, since it is made up of a series of 600 lines, 150 of each of the four lengths, was followed at each test period and training period.

Twenty-four *S*s were tested with this series with no announcement of "right" or "wrong," before and after seven training periods at least a day apart, in which "right" or "wrong" was announced about one second after each response. From start to finish no *S* ever saw any line that he drew, or had any practice other than that described above.

The percent of "right" responses in each training period and the percent which would have been "right" (if announcement had been made) in the early and late tests are reported in Table IV. Every *S* improved during the training, though the gain of *S* 41 was so small as to be uncertain.

The percent right rose from a median of 13 in the early test to a median of $34\frac{1}{2}$ in the first training period, and a median of $54\frac{1}{2}$ in the seventh. In the late test, when the *S*s were without any guidance and connection from the announcements of "right" and "wrong," the percent fell back to a median of $26\frac{1}{2}$, but was twice as large as in the early test. Sixteen of the 24 *S*s had more right in the late than the early test. The average gain was 12, with a probable error of 2.2.

A more accurate estimate of the influence of the "right and wrong" conceptions of the connections may be had by measuring each line drawn and computing the average errors in the early and late test for the four lengths. This we have done with the results shown in Table V. There is a reduction in the error for each length and for 18 of the 24 *S*s. The average reduction is nearly eight times its probable error.

If we could assume that mere repetition of such line-drawing with no difference in the effect of accurate and inaccurate responses would produce zero reduction in the error, the result of experiment B would be a second proof that the effects of a connection do work back upon it to alter its strength. It seems better, however, to check this assumption by experiment. Unfortunately, it has been possible to date to do this with only 6 *S*s. Each of them drew 5400 lines just as the *S*s of experiment B did, except that no announcement of

"right" or "wrong" was made at all. From the early to the late experiment three of them improved and three got worse. The gains were, however, greater than the losses, so that there was an average total reduction of 75 for these six, compared with 190 for the 24 *Ss* who had the effects of the "right" and "wrong." The probable error of the 75 is ±70. Consequently, the difference of 115 in favor of the

## TABLE IV

THE INFLUENCE OF EFFECT UPON DRAWING LINES WHEN BLINDFOLDED: The percent of "right" response in early and late tests and during the training itself.

| *S** | Percent Right | | | | | | | | | Gain: Late over Early |
|---|---|---|---|---|---|---|---|---|---|---|
| | Early Test | Training Periods | | | | | | | Late Test | |
| | | 1 | 2 | 3 | 4 | 5 | 6 | 7 | | |
| 16 | 1 | 38 | 48 | 49 | 52 | 56 | 54 | 61 | 21 | 20 |
| 17 | 28 | 47 | 56 | 69 | 61 | 65 | 68 | 67 | 21 | − 7 |
| 18 | 2 | 40 | 47 | 50 | 51 | 58 | 55 | 57 | 33 | 31 |
| 19 | 13 | 34 | 33 | 47 | 53 | 49 | 35 | 56 | 39 | 26 |
| 20 | 0 | 25 | 42 | 45 | 40 | 47 | 41 | 44 | 30 | 30 |
| 21 | 15 | 30 | 47 | 52 | 58 | 58 | 58 | 57 | 37 | 22 |
| 23 | 13 | 35 | 43 | 42 | 47 | 48 | 52 | 60 | 9 | − 4 |
| 25 | 1 | 37 | 38 | 44 | 44 | 44 | 55 | 60 | 28 | 27 |
| 26 | 16 | 46 | 50 | 43 | 49 | 34 | 51 | 54 | 28 | 12 |
| 27 | 12 | 37 | 43 | 45 | 35 | 45 | 49 | 60 | 36 | 24 |
| 28 | 12 | 33 | 38 | 39 | 50 | 30 | 46 | 42 | 11 | − 1 |
| 29 | 24 | 55 | 62 | 75 | 77 | 85 | 88 | 93 | 65 | 41 |
| 33 | 0 | 32 | 48 | 50 | 24 | 36 | 44 | 47 | 35 | 35 |
| 34 | 16 | 31 | 34 | 36 | 48 | 48 | 55 | 55 | 31 | 15 |
| 35 | 11 | 26 | 33 | 32 | 46 | 38 | 46 | 33 | 6 | − 5 |
| 36 | 0 | 0 | 18 | 41 | 43 | 50 | 46 | 47 | 20 | 20 |
| 37 | 6 | 41 | 53 | 51 | 54 | 61 | 53 | 65 | 24 | 18 |
| 40 | 24 | 33 | 34 | 37 | 32 | 37 | 41 | 46 | 28 | 4 |
| 41 | 28 | 39 | 40 | 40 | 36 | 46 | 40 | 41 | 25 | − 3 |
| 42 | 31 | 34 | 38 | 41 | 40 | 30 | 48 | 50 | 4 | −27 |
| 43 | 7 | 31 | 34 | 27 | 35 | 41 | 40 | 39 | 21 | 14 |
| 44 | 24 | 36 | 44 | 51 | 49 | 53 | 60 | 62 | 35 | 11 |
| 46 | 20 | 36 | 42 | 45 | 52 | 48 | 48 | 53 | 11 | − 9 |
| 48 | 13 | 26 | 38 | 50 | 53 | 46 | 54 | 48 | 9 | − 4 |
| | | 1003 | | | | | | | 1297 | |

*It may be of interest to know that *Ss* 16 to 29 were from 20 to 25 years old, and *Ss* 33 to 48 were 35 years old or over, averaging 42.

influence of effect has a large probable error (±74). There is one chance in seven that it may in the long run prove to be as low as 0.

From certain other experiments, and certain added facts about these experiments, which I shall not take time to present here, I am

TABLE V

THE INFLUENCE OF EFFECT UPON DRAWING LINES WHEN BLINDFOLDED

| S | Average Divergences | | | | | | | | Changes | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 3 in. | | 4 in. | | 5 in. | | 6 in. | | 3 in. | 4 in. | 5 in. | 6 in. | Total |
| | Bef. | Aft. | Bef. | Aft. | Bef. | Aft. | Bef. | Aft. | | | | | |
| 16 | 98 | 41 | 143 | 47 | 201 | 53 | 232 | 114 | − 57 | − 96 | −148 | −118 | −419 |
| 17 | 28 | 32 | 44 | 59 | 69 | 61 | 84 | 81 | + 4 | + 15 | − 8 | − 3 | + 8 |
| 18 | 87 | 27 | 125 | 36 | 130 | 49 | 141 | 47 | − 60 | − 89 | − 81 | − 94 | −324 |
| 19 | 59 | 33 | 96 | 29 | 113 | 37 | 131 | 38 | − 26 | − 67 | − 76 | − 93 | −262 |
| 20 | 129 | 38 | 190 | 38 | 222 | 57 | 250 | 52 | − 91 | −152 | −165 | −198 | −606 |
| 21 | 38 | 42 | 66 | 29 | 78 | 29 | 93 | 34 | + 4 | − 37 | − 49 | − 59 | −141 |
| 23 | 64 | 44 | 76 | 65 | 73 | 113 | 99 | 163 | − 20 | − 11 | + 40 | + 64 | + 73 |
| 25 | 111 | 22 | 150 | 33 | 208 | 64 | 262 | 64 | − 89 | −117 | −144 | −198 | −548 |
| 26 | 80 | 36 | 68 | 35 | 93 | 54 | 84 | 47 | − 44 | − 33 | − 39 | − 37 | −153 |
| 27 | 54 | 35 | 91 | 30 | 97 | 39 | 111 | 46 | − 19 | − 61 | − 58 | − 65 | −203 |
| 28 | 79 | 58 | 79 | 103 | 103 | 89 | 115 | 94 | − 21 | + 24 | − 14 | − 21 | − 32 |
| 29 | 62 | 18 | 71 | 22 | 65 | 25 | 66 | 22 | − 48 | − 49 | − 40 | − 44 | −181 |
| 33 | 212 | 27 | 210 | 23 | 275 | 34 | 336 | 40 | −185 | −187 | −241 | −296 | −909 |
| 34 | 84 | 27 | 87 | 34 | 90 | 57 | 101 | 81 | − 57 | − 53 | − 33 | − 20 | −163 |
| 35 | 63 | 56 | 103 | 80 | 126 | 97 | 146 | 116 | − 7 | − 23 | − 29 | − 30 | − 89 |
| 36 | 94 | 45 | 151 | 65 | 202 | 53 | 279 | 62 | − 49 | − 86 | −149 | −217 | −501 |
| 37 | 36 | 43 | 83 | 42 | 150 | 69 | 179 | 54 | + 7 | − 41 | − 81 | −125 | −240 |
| 40 | 50 | 32 | 57 | 48 | 64 | 62 | 76 | 58 | − 18 | − 9 | − 2 | − 18 | − 47 |
| 41 | 37 | 54 | 45 | 48 | 56 | 51 | 71 | 78 | + 17 | + 3 | − 5 | + 7 | + 22 |
| 42 | 58 | 76 | 56 | 122 | 71 | 165 | 70 | 189 | + 18 | + 66 | + 94 | +119 | +297 |
| 43 | 104 | 48 | 112 | 78 | 115 | 54 | 110 | 72 | − 56 | − 34 | − 61 | − 38 | −189 |
| 44 | 76 | 24 | 58 | 45 | 56 | 37 | 55 | 45 | − 52 | − 13 | − 19 | − 10 | − 94 |
| 46 | 33 | 46 | 45 | 93 | 73 | 93 | 86 | 143 | + 13 | + 48 | + 20 | + 57 | +138 |
| 48 | 75 | 80 | 79 | 70 | 101 | 98 | 91 | 106 | + 5 | − 9 | − 3 | + 15 | + 8 |
| Av. | | | | | | | | | − 35 | − 42 | − 54 | − 59 | −190 |
| P. E. of Av. | | | | | | | | | ± 5 | ± 7 | ± 9 | ± 11 | ± 25 |

of the opinion that the extension of experiment B and its control to more individuals and to more than the 4200 lines of training (or mere repetition with the controls) will strengthen the case for effect. This opinion may be somewhat justified to the reader by the fact that the 6 *S*s (of whom only three improved by mere repetition) all improved

when subjected later to experiment B; and improved by an average reduction of 143 ±27 compared with the 75 ±70 which repetition had brought.

Assuming that extensions of experiment B confirm our results in favor of a strengthening by "right" or a weakening by "wrong," or both, we have as in the case of the judgments of length, the question whether the influence acts directly on the connections themselves or indirectly by leading the *S*s to repeat some inner counterparts of the connections.

Under the conditions of the experiment, any actual repetition of the successful movement could not occur. The most that the *S* could do was to rehearse the inner "feel," or the cues which led him to make it; and there was little time for this between the execution of one movement and the demand for the next. What could be done was to retain or recall such a feel or such cues after a "right" and abandon or neglect them after a "wrong," and so get what benefit they gave in case the command was for a line of the same length as the previous one (as it was about five times out of six).

It seems probable that such deliberate retention or recall plays a part, but that there is also a more fundamental, general, and direct favoring of the successful connections, for two reasons. First, we have no evidence that a person improves any faster by trying deliberately to retain or recall such cues than by simply drawing line after line as he feels like doing. Second, it seems unlikely that cats, dogs and rats carry on any such deliberate reviews or rehearsals in the bulk of their learning.

By a more fundamental, general, and direct favoring, nothing mysterious need be meant. It may be so simple a thing as a longer duration of the connection, when the *S* hears "right" than when he hears "wrong." Such longer duration would presumably have the same strengthening influence as a larger number of repetitions of equal length.

Two features of the learning to draw lengths blindfold are striking and should be instructive. One is the slow and gradual nature of the process after the very early stage when the subject has (temporarily) got rid of any very pronounced tendency to draw the lines too short or too long. From the second to the seventh practice period, or from the first 150 to the last 150 of 900 lines of each sort, the gain in the number of right responses is only from an average of 42 to an average

of 54. The other is the great discrepancy between the ability under the immediate guidance of the announcements of "right" and "wrong" and the ability after these cease.

Both of these facts seem to me to bear witness to the importance of what may be called identifiability (or, more precisely, 'get-at-ableness') in learning. If the directions had been "Draw a parabola," "Draw a catenary," "Draw a cissoid," "Draw a cardioid," the *S*s, if they succeeded at all, would have probably quickly changed from 0% right to 100% right. They would also have maintained their ability without incessant guidance by "right" and "wrong." When a situation is identifiable so that a person can name it, or place it, or in anyway react to it, distinguished from other situations, and when a response is identifiable in the sense that the person can in any way make it and not any other response than it, the process of connecting any of his repertory of such responses with any of his repertory of such situations usually requires very few repetitions, and takes place very suddenly, suggesting an 'all-or-none' type of action.

The training in experiment B probably was efficacious partly in identifying the responses, more or less temporarily, and partly in strengthening their connections with the four commands. It is reasonable to suppose that the latter influence was much more permanent than the former. It is also reasonable to suppose that the failures of certain *S*s to improve were not due to a failure of the law of effect to act in their case, but to a failure of the response to become sufficiently get-at-able to be connectible with anything.

# A STUDY OF THREE VARIABLES IN MEMORIZING

By ELEANOR A. McC. GAMBLE, Wellesley College

The work here reported is only a small beginning of a very large undertaking. So large is the plan that the writer, though of good courage, has little rational hope of ever being able to fulfill it. Strictly speaking, no one else can complete exactly this piece of work, and the writer believes that few experimentalists have the patience to go far with parallel studies, though this statement is launched as a challenge. Her own work is to be compared to a large piece of timber into which one has chipped deeply enough to be able to see the grain of the wood. Though the timber may never be nicely carved into shape, yet it may seem worth while to discover its grain.

The purpose of the work is to determine the relative importance of three variables in the experimental procedure of the method of right associates (*Treffer- und Zeit-methode*) and, if possible, the interplay of their several effects. The procedure is exactly that which was elaborated by Müller and Pilzecker and is familiar to all investigators in the field of memory. Suffice it to say here that the method is as follows:

A series of the nonsense syllables (or words) is read to or by the subject a certain number of times at some uniform rate. Then, after a certain interval, during which the subject should make *no* review of the series, the experimenter presents to him every other syllable (usually the odd-number syllables) and requires him to name in each case the following (or preceding) syllable. The subject's reaction time is measured, whether his response be a syllable, right or wrong, or the words, "Don't know." The results are stated in terms of numbers of right cases (*i.e.* correct responses), partly right cases, wrong cases, zero cases, and of the average reaction-time for each set of cases.

The three variables studied in this work are length of series, rate of presentation, and number of presentations.

The work sprang out of a suggestion which was made by Professor Edwin G. Boring to Professor Horace B. English and which was shared by the latter with the writer. Professor Boring advised that the several values for each of the independent variables should be five in number, *i.e.* that the number of series-lengths, the number of different rates of presentation, and the number of presentations should be five. This suggestion entailed 125 different sets of series. If 10 be considered a sufficient number for each set, then it is necessary for the completion of the work with any one subject that he should learn 1250 homogeneous series under conditions which shall be uniform save for the variables studied. For extensive experiments in memorizing the writer can find as a sub-

ject only herself, a learner with marked peculiarities; and even with herself she has been far from able to complete the program. During each period of the investigation a few student subjects have been employed but they were introduced for the edification of the student experimenters. No one of them passed beyond the period of rapidly increasing practice, and their work is entirely neglected here.

Before the results secured with the writer are indicated, the following topics must be briefly considered: (*1*) the periods into which the investigation falls; (*2*) the nature of the series learned; (*3*) the values of the variables and the other important details of the experimental procedure; (*4*) the peculiarities of the learner.

(*1*) The periods of experimentation have been three. In all three periods student *E*s worked upon the writer as chief subject. When speaking of herself as *S* the writer will henceforth call herself 'G.' The work of *Period 1* continued during the whole of the academic year 1922-1923. G served for nearly an hour 5 times a week during the first, and 7 times a week during the second semester. The *E* was Miss Sue B. Stryker (Wellesley, 1923). The work of *Period 2* was confined to the first semester of 1923-1924. Miss Grace W. Moffat (1924) was the *E*. The work of *Period 3* fell in the spring semester of 1925. The *E*s were Miss Miriam Arrowsmith and Miss Vivienne Reinhart (1925). During the second period, G served 4 times a week, and during the third, twice a week for each *E*.

(*2*) The character of the syllables used can best be indicated by giving the following specimen of a 16-member series: *kike, rel, sim, wud, zax, hod, shouz, blune, peme, tace, kred, fum, loit, dal, quin, fide.* Every syllable begins and ends with a consonant sound or sounds and contains a central vowel or diphthong. The writer's complete outfit of syllables has been constructed according to definite rules suggested by those of Müller and Schumann but adapted to the phonetic usages of the English language.[1] Combinations of letters which spelled genuine words have not been excluded. The syllables in use in these experiments numbered about 4500.

The work required so very many and such long series that no great amount of time was spent upon refining them. Every syllable was typewritten upon a small card and the cards were shuffled and stirred about in a large box. From this box the experimenter drew given numbers of cards at random and arranged them in series in nearly the order in which they were drawn. Care was taken, however, that no consonant or vowel should be repeated in successive syllables and also that rhymes and meaningful combinations of syllables should be excluded. No syllable was used a second time in any period of the work until the whole supply had been drawn from the box.

(*3*) All presentation was aural. *E* read the series to the beats of a metronome set at eighty per minute. Thus the interval between the beats was ¾ of a

[1]Cf. G. E. Müller and F. Schumann, Experimentelle Beiträge zur Untersuchung des Gedächtnisses, *Zsch. f. Psychol.*, 6, 1894, 99-106. The writer has described her own syllables in, A study in memorizing various materials by the reconstruction method, *Psychol. Mon.*, 16, 1909, no. 43, 19-22. Since that description was written", the range of syllables has been enlarged, for example, by the use of such consonant beginnings as *bl* and *kr*.

sec., and the series most rapidly read were presented at this rate. Series more slowly read were presented one to every second beat, one to every third beat, and so on. Whatever the rate, the signal 'Now' was given two beats before the beginning of each presentation and two beats after the end of each presentation which was to be followed by another. The interval between the last presentation and the test-procedure was always three minutes. In determining for a series the order in which the odd-number syllables should be presented as stimuli, a cyclic program was used. After the beginning of each period of work this program was devised by the experimenter and was not known to G. Reactions were timed with an ordinary vernier chronoscope. *E* released the long pendulum as she pronounced the stimulus-syllable and the short pendulum at the instant of G's response.

For brevity in the following pages, series-length will be symbolized by *L*, rate of presentation by *R*, and number of presentations by *P*. The values of *L*, *i.e.* the number of syllables in the series, were, in the first period of the work, 16, 24, 32, 40, and 48. In the second period they were 12, 24, 36, 48, and 60. The reason for using such long series will be clear when the results are considered. In both periods the values of *P* were 1, 2, 3, 4, and 5, and the rates were one syllable to each beat of the metronome, one to two beats, to three, to four, and to five, *i.e.* one to three-fourths, to one and one-half, to two and one-fourth, to three, and to three and three-fourths seconds. It will be noted that in Period 2 the values of *L*, as well as those of *R* and of *P*, are related to one another as are the numbers 1, 2, 3, 4, 5.

In *Period 1* the *E* began with a series given under the easiest set of conditions, that is, with *L* at sixteen, with *R* at 'one to five,' and with *P* at five. The *E* then proceeded to give one series under each set of conditions, ending with the hardest, that is, with *L* at forty-eight, *R* at 'one to one,' and *P* at one. The combinations of *L*, *R*, and *P* were then repeated in reverse order. When the full cycle was completed a few combinations—especially combinations with *L* at 32 or with *P* at 3 or with *R* from one to three—were used a third time.

In *Period 2* the various combinations of *R*, *L*, and *P* were not taken in regular order but rather in an order determined by lot. No combination was repeated. Indeed, time did not permit the use of the full list. A series of 60 syllables requires nearly an hour for repeated presentation and for the test procedure. In the case of each series *S* was left to find out the combination for herself. This step was taken because G believed that in Period 1 she had "taken it easy" when she knew that a particular combination of *L*, *R*, and *P* was not such as to tax her capacity. Toward the end of the work, when it was evident that not all combinations could be used, G directed that the full list of combinations containing *L* 36 should be completed. (Thirty-six was the median series-length.)

In *Period 3* in order that the student *E*s might have data which could be more easily handled, the writer selected eighteen combinations of *L*, *R*, and *P* out of the one hundred and twenty-five originally scheduled for *Period 2*. In the experiments made by Miss Reinhart, *L* was fixed at thirty-six; the values of *R* were only one to five, one to three, and one to one; and the values of *P*, one, three and five. In Miss Arrowsmith's experiments *P* was fixed at three whereas the values of *L* were only twelve, twenty-four and forty-eight and those of *P*, one, two and four. Cyclic programs for the combinations were used by both *E*s.

(4) The writer has several times in print described G as a learner but the following points must be reiterated here. (a) This subject has great facility in memorizing, partly inborn and partly a matter of practice. Laboratory exigencies, vanity, and waning eyesight have combined to prevent this practice from lapsing. It is not confined either to 'rote' or to 'logical' learning nor to any particular material. (b) G learns in terms both of visual imagery and of internal speech, but the former is usually in the focus of consciousness whereas the latter is more or less marginal. (c) Her verbal visual images are peculiar in that they are not representations of words, written or printed, but are blotches of color of synaesthetic origin. (The subject has colored hearing in a very marked degree.) The blotches are ill-defined oblongs and generally contain more than one color. Upon them the image of a letter will often flicker. When G is reciting a series of syllables which she knows well, she visualizes the oblongs of color in succession and almost simultaneously finds herself making her speech reactions. When she is attempting to recite a series only partially learned she tries to anticipate the color spots and as she does so internal speech becomes focal. (d) If the material consists of a list of syllables or disconnected words, G projects the units into a very definite pattern in her mental field of vision. For at least seven years this pattern has taken the form of columns of twelve with three groups of four units in each column. It is into this pattern that the spots of color are projected. In series containing more than twelve members the columns are arranged from left to right with the thirteenth a spot exactly to the right of the first, the twenty-fifth exactly to the right of the thirteenth, and so on. Reflection will show that this scheme enables G to make associations between syllables which may stand at a considerable distance from one another in the series. If the syllables have or suggest meanings, then concrete visual images, vaguely outlined, may hover about the spots in the pattern. (e) When, in the test procedure of the method of right associates, a stimulus-syllable is given, G at once projects the corresponding spot of color into a particular position in the pattern. As soon as she does this, the following syllable will usually appear—evidently evoked not merely by the direct association between it and the stimulus-syllable but also by its association with a particular place in the pattern. But it should be noted that, except as the syllables are projected into it, the pattern consists simply of a brownish field whose parts have spatial relations which G is constantly stressing. At any one moment the syllables visualized in their spatial relations are very few. During the process of recall it is as if a spot-light were flashed here and there in a garden full of colors. (f) G does not count the syllables as they are presented but simply projects them into the pattern. When she knows a series she can very quickly give the number of any syllable in it, but she gets this number by swiftly computing the number of the position of the syllable in the pattern. (g) G makes a considerable use of associations extrinsic to the series and other than those just described, but is by no means dependent upon such 'mnemonic devices.' (h) For her the difficulties created by rapid presentation are simply the difficulty in properly apprehending the syllables and the difficulty of projecting them into their proper places quickly enough to keep pace with the reading.

G's colored hearing undoubtedly assists her in memorizing for she often impresses part of the series upon herself by noting similarities, contrasts, or "pretty effects" among the colors. Nevertheless, this assistance is merely in-

cidental in comparison with her practice in projecting visual images into a definite spatial pattern. It is in this habit that the *S*'s technique essentially consists.

## Results

The results of the experiments will be discussed period by period. Throughout the discussion let *N* stand for "number of right associates possible" and *RC*% for "percentage of right cases (associates)."

*Period 1.* Here the discussion falls into two parts. These are (*1*) the comparison of the results (*a*) for series of different lengths—regardless of *R* and *P*, (*b*) for series presented at different rates—regardless of *L* and *P*, and (*c*) for series presented different numbers of times—regardless of *L* and *R;* and (*2*) the comparison of the results obtained with different combinations of *R*, *L*, and *P*.

(*1*) *Comparison of results:* (*a*) *series of different lengths.* For *L* 16, *L* 24, *L* 32, *L* 40, and *L* 48, *N* was respectively 408, 624, 992, 1060, and 1224. The percentages of right cases were 83, 80, 83, 73, and 74. The series-lengths are related as are the numbers 100, 150, 200, 250, and 300. The numbers of right cases were related approximately as are the numbers 100, 96, 100, 88, and 90. *L* 32 is double *L* 16 but the corresponding percentages of right associates were exactly the same. When *L* trebles, *RC*% diminishes by less than one-eighth. It should be noted that the number of sequences learned was *absolutely* larger and *not* smaller in the case of the longer series. In detail the facts are these: With *L* 16, 6.7 sequences were learned (on the average) out of a possible 8; with *L* 24, 9.6 were learned out of a possible 12; with *L* 32, 13.2 out of 16; with *L* 40, 14.5 out of 20; and with *L* 48, 17.5 out of 24. These figures show the reason for the selection of series-lengths as great as forty and forty-eight.

(*b*) *Series presented at different rates.* With *R* at 1 to 5, 1 to 4, 1 to 3, 1 to 2, and 1 to 1, *N* was respectively 844, 816, 976, 856, and 816. *RC*% was respectively 89, 84, 84, 76, and 53. As between one syllable to 4 metronome beats and one to 2, the rate of presentation doubled. The percentage of right cases fell off by less than one-tenth. Between 1 to 2 and 1 to 1, *R* again doubled but here *RC*% fell off by about one-third. When the rate quintupled, *RC*% decreased by a little more than two-fifths. It is clear, however, that speeding up the rate of presentation produced no tremendous effect on the

number of right associates until the rate reached 1 to 1, that is one syllable to three-quarters of a second—a rather fast rate for aural presentation of nonsense syllables.

(*c*) *Series presented different numbers of times.* With *P* 5, *P* 4, *P* 3, *P* 2, and *P* 1, *N* was respectively 844, 816, 996, 816, and 836. *RC*% was 92, 88, 84, 73, and 48. When the number of presentations fell from four to two, the percentage of right associates decreased by a trifle more than one-sixth. But when *P* fell from two to one, *RC*% fell by more than one-third. When *P* fell from four to three, *RC*% fell by only one twenty-second.

Scanty as these results are, they have a positive significance just as a good performance on an intelligence test shows something positive. If a subject *does* succeed even once in a performance, then we know that he *can* succeed again (unless his first success was purely due to accident). *It is evident that, for a learner of this type, the kind of performance required in the method of right associates does not become proportionally difficult as series-length increases, as rate of presentation increases or as number of presentations decreases. It is also evident that number of presentations makes somewhat more difference than does rate of presentation, and that rate makes more difference than does length of series.*

*S* gave extremely few wrong responses. Hence nothing important can be gleaned from the discussion of the relative numbers of wrong and of zero cases. Wrong responses were usually due to some extrinsic association. G is too familiar with nonsense syllables to have great difficulty in apprehending them correctly. But if the sequence *yil koit* suggests yellow cat, then any *S* is likely to respond to *yil* with *cat* even if he instantly corrects himself. In all sets of series and in all periods of the work, G's average reaction-time for right associates was about three seconds—usually somewhat less. Variations were so small that detailed discussion of them here would be a waste of time.

(*2*) *Comparison of results obtained with different combinations.* Of the results obtained with the various combinations of *L*, *R*, and *P*, a notion may be given by massing the results of the two or three series obtained with each combination and then considering the combinations which yielded more than 90% or less than 50% of right cases. The data for the former are presented in the following scheme.

PERCENTAGES YIELDING MORE THAN 90% OF RIGHT CASES

| | | $L$ 16 $P$ | $L$ 24 $P$ | $L$ 32 $P$ | $L$ 40 $P$ | $L$ 48 $P$ |
|---|---|---|---|---|---|---|
| $R$ | 1 to 5 | 5, 4, 3, 2 | 5, 4, 3, 2 | 5, 4, 3 | 5, 4, 3 | 5, 4, 3, 2 |
| $R$ | 1 to 4 | 4, 3, 2 | 5, 4, 2 | 3, 2 | 5, 4, 3 | 5, 4 |
| $R$ | 1 to 3 | 5, 4, 3 | 5, 4, 3 | 5, 4, 3, 2 | 4, 3 | 5, 4 |
| $R$ | 1 to 2 | 5, 4, 3 | 5, 4 | 5, 4 | 5 | 5 |

This scheme is to be interpreted as follows: with $L$ 16 and with $R$ at 1 to 5, the sets of series which yielded over 90% of right cases were those with $P$ at 5, 4, 3, 2; with $L$ 24 and $R$ at 1 to 5, the sets were those with $P$ at 5, 4, 3, 2; and so on.

The data for the series which yielded less than 50% of right cases can best be presented in words. With $L$ 16 the only sets which yielded less than 50% were those with $R$ at 1 to 2 and $P$ at 2, and with $R$ at 1 to 1 and $P$ at 1. With $L$ 24 and with $L$ 32 the only sets in which $RC\%$ was less than 50 were those with $R$ at 1 to 1 and $P$ at 1 or at 2. With $L$ 40 and $L$ 48 no set in which $P$ was 1 yielded 50% of right cases and in addition the set in which $R$ was at 1 to 1 and $P$ was 2 fell below the line.

From these data the following inferences may be drawn.

(*a*) Speeding up the rate of presentation did not work havoc with the learning of the longer series although it, of course, affected them more than it did the shorter. No series of any length, long or short, yielded more than 90% of right cases when $R$ was at 1 to 1. But series of all lengths yielded over 90% when $R$ was as rapid as 1 to 2. 'The following figures may be added to the data just given. With $R$ at 1 to 1, $RC\%$ for $L$ 16 was 61; for $L$ 24, it was 63; for $L$ 32, it was 56; for $L$ 40, it was 49; for $L$ 48, it was 47. (Here variations in $P$ are disregarded.) It may be remembered that $RC\%$ dropped only from 83 to 74 when $L$ rose from 16 to 48 and when series presented at all five rates were included. The difference between 83 and 74 is less than one-ninth of 83 whereas the difference between 61 and 47 is more than two-ninths of 61. Nevertheless, the drop in right cases with increasing series-length was not steadily sharp even with the fastest rate of presentation.

(*b*) Cutting down the number of presentations was, as might be expected, a more serious handicap in the case of the longer than it was in the case of the shorter series, and in the case of the longer it was more serious than was speeding up the rate of presentation. No series yielded over 90% of right cases with $P$ at 1. But with $L$ over 24, only one set of series yielded over 90% even with $P$ at 2. This

set is that given with $L$ at 48, and with $R$ at 1 to 4. With $L$ at 40 or at 48, no set gave 50% of right cases, no matter how slow the rate of presentation. But even with $L$ at 40 or at 48 and with $R$ at its maximum, no series fell below the 50% line unless $P$ was less than 3. The following figures may be interpolated. The percentages of right cases with $P$ at 1 and for all rates combined run as follows: for $L$ 16, 65%; for $L$ 24, 55%; for $L$ 32, 59%; for $L$ 40, 36%; for $L$ 48, 40%. It is clear that the drop in right cases with $P$ at 1 is by no means steady as $L$ increases and that it is not catastrophic. But in passing from $L$ 16 to $L$ 48 in the series in which $P$ was 1, we find a drop (from 65% to 40%) which is sharp in comparison with the drop (from 83% to 74%) encountered in passing from $L$ 16 to $L$ 48 when all series of these lengths are combined, whatever the value of $P$. It is quite evident that series-length was in these experiments the least important factor in the learner's task.

(*c*) Neither raising the rate of presentation nor reducing the number of presentations produced a very drastic effect unless $R$ was raised to 1 to 1 or $P$ was reduced to 1. But the data thus far presented with regard to the combinations of $L$, $R$, and $P$ do not show clearly whether raising $R$ or reducing $P$ has the more mischievous effect upon percentage of right associates. Hence, the following data should be added. For all series with $R$ at 1 to 1 and with $P$ at 2, $RC\%$ was (collectively) 43. For all with $R$ at 1 to 2 and with $P$ at 1, it was 38. For all series with $R$ at 1 to 1 and with $P$ at 5, $RC\%$ was 76, but for all with $R$ at 1 to 5 and $P$ at 1, it was only 60. For the combinations of $R$ at 1 to 1 with $P$ 5, $P$ 4 and $P$ 3, $RC\%$ was 64, whereas for the combinations of $P$ 1 with $R$ at 1 to 5, $R$ at 1 to 4, and $R$ at 1 to 3 it was only 54. Such figures as these confirm the earlier conclusion that $P$ is a somewhat more important variable than $R$.

(*d*) G learned little better at the rates 1 to 5 and 1 to 4, than she did at the rate 1 to 3. The slower rate, 1 to 5, which was equivalent to one syllable to three and three-fourths seconds undoubtedly wasted time.

(*e*) Probably a fifth repetition was often wasted, for G accomplished very little more with five repetitions than with four.

*Period 2.* Here the results are extremely scanty and are also unsymmetrical since only 93 out of the 125 possible combinations of series-lengths, rates and numbers of presentations were used. Discussion will, therefore, be confined to the comparison of the results

obtained (*a*) with series of different lengths, (*b*) with series presented at different rates, and (*c*) with series presented different numbers of times. In every case two of the three variables—*R*, *L*, and *P*—will be neglected.

(*a*) In this period the series-lengths used were 12, 24, 36, 48, and 60. For series of these lengths *N* was respectively 102, 204, 450, 384, and 540. The corresponding percentages of right cases were 85, 81, 77, 76, and 71. The values of *L* are related to one another as are the numbers 20, 40, 60, 80, and 100. The corresponding percentages of right cases are related as are the numbers 100, 95, 91, 89, and 84.

(*b*) For *R* at 1 to 5, at 1 to 4, at 1 to 3, at 1 to 2, and at 1 to 1, *N* was respectively 246, 354, 324, 390, and 366. *RC%* was respectively 85, 83, 87, 76, and 53. The values of *R* are related as are the numbers 100, 80, 60, 40, and 20. The percentages of right cases are related as are the numbers 100, 98, 102, 89, and 62. It should be noted that G learned actually more with *R* at 1 to 3 than she did with *R* at 1 to 5.

(*c*) For *P* 5, *P* 4, *P* 3, *P* 2, and *P* 1, *N* is respectively 366, 318, 336, 324, and 348. *RC%* is respectively 93, 93, 86, 69, and 38. The values of *P*, like the values of *R*, are related as are the numbers 100, 80, 60, 40, and 20. The percentages are related as are the numbers 100, 100, 92, 74, and 42. Here it should be noted that four presentations appear to have accomplished as much as did five.

The results of the second period of experiments confirm those of the first at every important point. Increase in the length of series raised the absolute number of right associates and did not diminish even the percentage very greatly. *R* was a more important variable than *L*, and *P* than *R*, but increase in *R* and decrease in *P* produced no tremendous effect upon the percentage of right associates until *R* reached 1 to 1 and *P* fell to 1. Even with *R* at 1 to 1, the percentage of right associates is over three-fifths of the percentage obtained with *R* 1 to 5; and even with *P* at 1, the percentage is more than two-fifths of that obtained with *P* at 5.

*Period 3.* Here only 18 combinations of the variables *L*, *R*, and *P* were used. In 9 of them *P*, and in the other 9 *L*, remained constant. Hence, it is possible to throw the results into the following compact scheme.

PERCENTAGES OF RIGHT ASSOCIATES

*P* 3

| | *L* 12 | *L* 24 | *L* 48 | All *L*s |
|---|---|---|---|---|
| (*N*) | (42) | (84) | (168) | (294) |
| Rate, 1 to 4 | 95 | 96 | 92 | 94 |
| 1 to 2 | 93 | 88 | 88 | 89 |
| 1 to 1 | 64 | 61 | 53 | 57 |
| All rates | 84 | 82 | 78 | |

*L* 36

| | *P* 5 | *P* 3 | *P* 1 | All *P*s |
|---|---|---|---|---|
| (*N*) | (90) | (90) | (90) | (270) |
| Rate, 1 to 5 | 100 | 98 | 68 | 87 |
| 1 to 3 | 100 | 87 | 53 | 80 |
| 1 to 1 | 60 | 54 | 28 | 50 |
| All rates | 89 | 80 | 47 | |

The first part of this scheme may be translated as follows. Among the series presented three times, those of *L* 12 could have yielded 42 right associates at each rate of presentation, those of *L* 24 could have yielded 84, and those of *L* 48, 168. In other words, seven series of each length were given at each rate. Altogether, the series of the three different lengths could have yielded 294 right associates at any one rate. As a matter of fact, with *R* at 1 to 4, *RC*% for *L* 12, *L* 24, and *L* 48, was respectively 95, 96, and 92.

Long discussion of the figures displayed would consist simply in laboring points already made. We find again in *Period 3*, as in *Periods 1* and *2*, that a great increase in series-length meant only a slight decrease in the percentage of right cases. Again, as in the earlier periods, quickening the presentation rate and cutting down the number of repetitions produced no great effect until the rate became extremely fast and the repetitions were reduced to one. In the experiments with *L* 36, *R* does indeed seem to be a more important variable than *P* but this showing is due to the fact that the experimenter had not yet learned to read the syllables both rapidly and correctly.

## FINAL REMARKS

No summary of conclusions is needed but certain final remarks should be made. Learning syllables by pairs as one does in the method of right associates, is supposed to be a much easier task than learning the series in such a way as to be able to recite it from beginning to end. G, however, could usually recite in order all the syllables of any series in which all her responses had been correct. In fact, she frequently proved her ability to recite such a series backwards. For the recitation she, of course, took her own time. The

relative difficulty in the performance required by the *Erlernungsmethode* as compared with the *Treffermethode* lies for G in the fact that she must recite a series of syllables at a prescribed rate and not in the fact that she must reproduce them in order.

G's success in learning syllables is no doubt due in part to native capacity for memorizing (evident in very early childhood), in part to long practice, and in part to the composure and genuine pleasure with which she enters upon her task. In some measure it may depend upon her fertility in extrinsic associations with parts of the series. But the fact that she could learn so nearly the same proportion of syllables in series of very different lengths is undoubtedly due to her practice of projecting visual imagery into a scheme of very definite spatial relations. This practice multiplied associations with the series-members, not merely place associations but also associations between syllables which did not stand near one another—except in the pattern. The vividness of the pattern is demonstrated by the *S*'s ability to recite series backwards. Long series meant a complicated scheme; they did not mean a pervasive confusion with emotional agitation. A minor factor in equalizing results for series of different lengths was the mettle with which the subject attacked the longer series and the lazy self-confidence with which she encountered the shorter.

In view of the *S*'s ability to memorize, it is not at all surprising that changes in presentation rate and in number of repetitions made little difference in the output of right associates until the rate was very rapid or the number of presentations was reduced to one. As to rate, it is now a commonplace of psychology that fast repetitions accomplish more in proportion to the time spent upon them than do slow. Moreover, it should be noted that it is very probable that G gave high-level attention to a syllable-sequence for little more than one second—at most one and one-half seconds—even when *R* was slower than one syllable to one and one-half seconds. The remainder of the longer intervals she used largely in 'looking about' in her pattern. This shows that experimenters have no way of gauging exactly the relation of different rates to a subject's mental activity.

It is also a well-known fact that a learner memorizes more in the first two repetitions than in any one additional repetition. The fact that G learned as much as she did learn with a single repetition is, however, to be traced to her pattern. She was ungrateful enough to feel that she would have learned even more if her unpractised *E*s had pronounced the syllables better.

It is very desirable that some one should repeat these experiments with a subject who has a facility and practice in rote memorizing which is comparable to G's but *who visualizes little.* The writer has even now in the laboratory an undergraduate student whose facility in rote learning rivals G's. But even if the services of this person could be secured, she visualizes too much to make a good foil for G. If one could find a trained subject with sufficient patience to carry him through the ordeal of such experiments, great facility in learning would not be needed. One could confine oneself to a list of rather short series. But experiments made upon persons whose practice is rapidly increasing are worthless. For her part, the writer is tempted to begin the whole study over again with rates of which one syllable to three seconds would be the slowest. She is thankful to say that, so far as memorizing goes, G's natural force is not abated.

# LEARNING AS IMPROVEMENT

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Learning has been defined in many ways. In this paper we shall consider it as an improved method of behavior which includes both the satisfaction of a creature's needs and the solution of its problems. But what is an improved method of behavior? Does this evaluation of behavior come from without—is it extraneous to the behavior? Or is it inherent in the behavior itself? General theories of value that rest upon extraneous principles require preconceptions of what is right, good, and satisfying in behavior. But is there no characteristic of improvement which can be described in terms of behavior itself? If there is, must not this characteristic be the basis of any theory of values, whether subjective or objective in its reference? Instead of considering first the various theories of rightness, goodness, and satisfaction advanced by philosophers, let us direct our attention to right action, good action, and satisfying states of affairs. Perhaps we shall find that behavior itself reveals a characteristic improvement which requires no preconceived scale for its evaluation.[1]

The achievements of a child in learning to draw pictures involve various changes in the coördination of fingers, hand, wrist, and arm; and also the participation of vision in guiding the hand and controlling the shapes that are drawn. The first achievements of the child are crude, both as regards technical skill and the picture that is drawn. But, as the child goes on, an improvement is made in the clearness of his line and in the smoother rhythm of his movements. His drawing, both as act and achievement, takes on more definite form as a *whole*. What we as observers record as a more skilful performance of the child is indeed a better coördinated and more rhythmical behavior in producing a picture, the perfection of which is a true counterpart of the kind of effort expended in its creation.

We have chosen a complicated example of improvement; for the ultimate perfection of drawing is only found in those rare achievements which we term works of art. Furthermore, works of art have no single standard of the highest value, but various standards. De-

[1]This paper is a chapter from a forthcoming book to be entitled *Philosophy and Education.*

grees of improvement in drawing pictures may be indicated in several more or less distinct ways, while beauty of design, representative verisimilitude, and impressionistic suggestiveness can be variously estimated. Nevertheless, so much may be said of any improvement in behavior: it is always marked by a greater coördination or smoothness of action which records itself in the product or achievement of the actor.

Generalizing, we may say that learning consists in coördinating the members, or smoothing out the 'kinks' in a course of action, and that this perfection is observable both in the action itself and in its product. If, then, improvement may be registered and described in terms of behavior itself, we have no need to seek first the formal theories of rightness, goodness, and satisfaction. Instead, we may begin with the concrete processes of living, and draw our theoretical conclusions from these.

It would be a wasteful procedure, however, to try to catalogue the diverse acts and achievements of a human being without the aid of some guidance in classification. This guidance can be had from psychology, the business of which science it is to study the fundamental types of behavior.

Psychology teaches that behavior falls into two main categories: unlearned or instinctive behavior, and learned or acquired behavior. Furthermore, learning is either perceptual or ideational. Primitive unlearned behavior is the basis of all learning, and ideational learning has its origin in perceptual learning. We have, then, for our guidance, not only a division into types of behavior, but also a genetic sequence. Primitive behavior owes its pattern to two things: first, the organized structure of the animal together with its differential sensitivity; and, secondly, an environment conducive to the existence of the organism as a living creature. An organism is by nature capable of selecting what it needs from its environment, but the environment must be ready to supply these needs in a certain prearranged manner; otherwise the organism will perish. Instead of being a touch-and-go affair, unformed and arbitrary as to its consequences, organic life, even in its very incipiency, is somehow patterned. The course of this behavior and the pattern of its achievement depend both upon the organism as such, and upon the environment as a pre-arranged source of supply for the organism's varied needs. Pre-arrangement is thus characteristic alike of the creature and of its surroundings.

This statement may seem dogmatic, for a contrary belief is widely held—a belief that all behavior reduces itself to physico-chemical changes, each limited and discrete, so that what appears as a course of behavior—as a pattern of response—is in reality an aggregation of separate processes, each one compulsory and for itself. We cannot deny that behavior is capable of reduction to physico-chemical processes, but we must deny that these processes are fundamentally separate. What we maintain is that, when rightly stated, the problems of bio-chemistry are a counterpart of the problems of the total response of the organism, and that the discreteness of a certain phase of this response is as artificial when stated in terms of physics and chemistry as it would be if it were stated in terms of behavior. In other words, the behavior of an animal is never a mere summation of individually independent acts, and the chemistry of an organism never warrants the supposition that processes like digestion, respiration, innervation, muscular contraction, etc., are attributable to the simple reaction of particles of matter one upon another. While we here can make no attempt to adduce the organic physics and chemistry which would substantiate this statement, it should be noted that our assertion is not an attack upon modern theories of physics and chemistry, but is directed against certain unwarranted conclusions that have been drawn by psychologists, sociologists, and educators.

What we propose to maintain is, briefly, this: that an adequate description of behavior will reveal characteristics of integration rather than those of discrete parts in a merely additive formation. If this statement be true, the physical and chemical counterparts of behavior must give warrant to it. If, on the other hand, our assertion be false, the error should reveal itself, not only in contradictions with physical science, but also in the inconsistency of our philosophy of behavior.

For the present, our assertion rests upon a common-sense philosophy of behavior. Turn as we will from one event of behavior to another, the criterion of 'improvement' on the way to 'perfection' is everywhere manifest in a certain characteristic of behavior for which the term *integration* is appropriate, whereas the idea of a mere addition or subtraction of units of behavior is inappropriate. It is therefore to this self-regulating integration of behavior that our notice is first drawn when we set out to discover the ways in which improvement takes place. Common-sense observation will then show

that these ways of improvement, which satisfy the needs and solve the problems of living things, are four in number: I. Differentiation; II. Assimilation; III. Gradation; and IV. Redefinition.

## I. Differentiation

Differentiation is any process of distinction in which a more integral or 'perfect' form sets itself off from the less integrated ground from which it arises.

In the neo-natal stage of infancy the behavior of nursing differentiates itself in accordance with this principle. In a child, sucking is almost the only well-developed form of behavior that is functional from birth. As Bertrand Russell writes: "When a child is engaged in this occupation, it feels at home with its new environment. But the rest of its waking life is passed in a vague bewilderment, from which relief is found by sleeping most of the twenty-four hours."[2]

This differentiation of an integrated behavior takes place under the conditions of internal need of nourishment and external presentation of the nipple at or near the mouth. Abortive sucking is also practiced upon the fingers and other objects of suitable size, but the complete act is hard to elicit when the child is not hungry, and the self-regulatory performance of the act is most perfectly achieved when milk of the right consistency and flavor is being supplied.[3]

While the act of nursing is not quite perfect the first time a child takes the breast, perfection comes so rapidly, and with so little evidence of trial and error, that we commonly call it an 'instinctive' action which is capable of functioning without being learned. The distinction between instinct and learning has, however, only a relative significance, because all instinctive acts are subject to improvement, and there is no learning that is not based upon some original capacity of behavior. Learning depends upon conditions which favor either the appearance of a new pattern of response, or the improvement of an old one. Logically, the differentiation which thus takes place whenever a more or less integrated form emerges, either suddenly or gradually, from a less integrated ground of behavior is a dichotomous opposition. The integral act $A$ is set off from the unintegrated residuum *not-A*. Certain boundaries are drawn, and a certain structure accrues to the act differentiated. But the opposition is not

[2]Cf. B. Russell, *On Education*, 1926, 70.
[3]Cf. the author's *Psychology and Education*, 1926, 8 ff.

complete. Integration begins with the nucleus of the act, and spreads, as it were, to the periphery or contour which differentiates the act from its ground. Thus the dividing line that separates an act from other irrelevant movements lacks the primary importance of a logical dichotomy, since no definite separation need exist at the outset. An original differentiation of behavior is not as a rule clean-cut; it may carry along with it variables that do not properly belong to the act, and it may contain features of behavior that have not as yet been integrated into the act as a whole.

What we observe in a nursing infant is that it sucks and swallows its milk with a certain degree of rhythmical regularity. But sometimes the child loses hold of the nipple; sometimes it coughs and chokes; sometimes its hands and legs are agitated. As the act of nursing perfects itself it becomes more and more stereotyped; irrelevant movements are less frequent, and the relevant movements attain a machine-like accuracy. In other words, the contours of the behavior define themselves as the integration of the act is improved.

The differentiation here considered is attributable not only to external conditions affecting the organism by chance and design but also to internal conditions that ripen with the growth of the organism, and seek such expression as their growth demands. In this sense, differentiation is neither exclusively biological nor physical, but biophysical in its nature. It is neither the formal logic of rational classification, nor the logic of natural objects and events, but the *bio-logic* of behavior in which the organism and its environment both participate. We shall be misled if we seek to explain the differentiations of behavior in terms of rational preconceptions on the one hand, or in terms of mathematical physics on the other. Whatever our explanation owes to either of these methods of thinking is derivative from psycho-biology. Both logic and mathematics have their origin in the behavior of living creatures. Accordingly, in the study of behavior we must start with a naïve description of events, in which the forms of logic and mathematics are implicit but not as yet explicit.

Starting with the most primitive manifestations of a living creature, we should endeavor to describe its behavior by putting ourselves at the level of the creature itself. Later we may seek to verify our description in terms of mathematics and logic; but we must be careful not to prescribe behavior in these terms, because a primitive form of behavior is never the clean-cut entity that certain logical

and mathematical methods of treatment presuppose it to be.[4] We have, however, the fact that an act of behavior like nursing holds itself together with a rhythmical sequence quite different from the state of 'vague bewilderment' which characterizes most of the waking moments of a new-born infant, and this indicates some inherent unity and cohesion. We may therefore believe that a genetic approach to the problem of organic differentiation will furnish its own criteria for what may later become rational units, without the further assumption that these units are in any real sense there to begin with.

## II. Assimilation

Assimilation is a method of improvement in learning that presupposes differentiation. The most primitive type of differentiation is a coarse and ill-defined particularization of an act or event. Assimilation, however, is the integration of two or more particulars into a new whole. Thus differentiation is genetically prior to assimilation. But that does not mean that assimilation involves a higher degree of improvement than differentiation. The product of an assimilation can be as coarse and ill-defined as any product of differentiation.

Furthermore, differentiation and assimilation occur either suddenly or gradually. In the one case, the new form snaps out of the old, as when we suddenly perceive a bird in a tree, or when we suddenly 'get the hang' of an act. In the other case, the conditions under which a particular emerges from its ground, or an act perfects itself by the assimilation of several previously learned parts, are not all given at once. Consequently, we have the effect of gradual learning without being able to determine any point at which differentiation or assimilation is complete. The differentiation of finger-movements in an act of manual dexterity is a slow process. Improvement in learning of this kind is gradual, and sometimes fitful, as can be seen from the 'learning curves' with their zig-zag patterns and plateaus which record these acts. But a differentiated particular may stand out suddenly with great clearness and precision, as it does whenever we give special heed to the movement of a single finger. Likewise, in the case of assimilation, two particular acts or events may slowly and gradually coalesce under the influence of repetition, or they may go together with surprising suddenness to form a new effect.

[4] New methods are being devised in logic and mathematics in order to account for these less 'clean-cut' phenomena.

Again, although differentiation and assimilation are logical opposites, bearing the implications of analysis and synthesis respectively, they can take place simultaneously in behavior. The integration of behavior is at once a process of differentiating and assimilating particulars. Paradoxical though it may seem, particulars acquire precision and lose individuality as the process of improvement goes on, so that in the end we have the appearance of a smoothly-working machine, each part of which is clearly defined, although it functions in articulate unity with every other part.

This picture of the improved end-result of a skillful performance has popularized the erroneous view that all behavior is mechanistic. While it is quite true that the particulars of behavior can be separately discerned, and their reciprocal articulations separately studied in the end-result of any skillful act, it by no means follows that these particulars were there to begin with, only awaiting assemblage like the various parts of a machine. On the contrary, nothing of the kind is 'there to begin with;' instead, there is merely a vague pattern of movement, within the uncertain bounds of which the differentiation of particulars and their assimilation are taking place in a single process of behavior.

Our failure to make education effective is largely attributable to a perversity of thought which insists on analyzing the end-result into its elements, after which a method of education is devised in accordance with the principle of assembling these elements to make a whole. We forget that these elements originally have no independent existence, but attain their separateness and independence only at the highest stages of learning and development. No infant can give heed to the movement of a single finger, or 'pay attention' to a mere spot of color. Yet the infant can give heed to its wants, and to the general pattern of their fulfillment; it can also heed and recognize the—to us, quite complicated—features of it mother's face and person.[5]

Another point, often overlooked or misunderstood, concerns the parts played by past experience and recognition in the improvement of behavior. It is sometimes assumed that we can learn only with reference to previous learning: the process is thought of as one of gradual accretion. This is not necessarily the case. We have already noted that differentiation and assimilation can be either gradual or sudden. While past experience, or memory, constantly exercises a

[5]Cf. K. Koffka, *The Growth of the Mind*, 1924, 133 ff.

powerful influence which makes learning appear for the most part gradual, creative acts also occur as true mutations. The essential feature of behavior is its form and not its content. It is forms and not contents that appear and improve themselves in the course of experienced behavior. The creation of a new form by differentiation and assimilation never can be regarded as a mere product of past experience; for all that past experience can do is to set conditions under which new forms may arise, either gradually or suddenly; and this function of experience is in no wise different from the biological functions of organic structure and sensibility with which the creature is endowed.

Frequently we make too sharp a distinction between the influences of heredity and environment, between native and acquired abilities. Appropriate for descriptive purposes as these conditions are, environmental conditions and experienced behavior can take effect only on the living, growing tissues of the organism. Furthermore, since form and not content must be emphasized in clarifying the modes of improving behavior, there is no definite line separating the creature from its environment, or the present situation from the past events that bear upon it. Such separations, when they are made, are based upon distinctions of content and not upon distinctions of external form. It is quite true that the physical environment takes on forms in which the organism does not participate. There are likewise forms of organic life, morphological, physiological, etc., unconnected with the surroundings of the organism. But when we speak of behavior, and especially of the improvement of behavior, we have reference neither to physics nor to biology, but to the self-regulatory interactions between situations and responses; although it is quite true that these interactions are subject to a strictly scientific definition in terms of physics and biology.

But, even so, when our attention is primarily directed on behavior, we are apt to attach what Whitehead calls a "misplaced concreteness" to our terms *situation* and *response*. Thus we sometimes find behavior treated as if it were composed of two totally disparate parts: the situation or stimulus, and the response or reaction. This distinction, however, is not all-important. If it were, we should be justified in reducing the stimulus to physics, and the reaction to biochemistry. When we did so, the behavior with which we are concerned would disappear. It is the form of behavior which interests us, and this form is something dynamic and rhythmical, with a 'situation' as its beginning stage, and a 'response' as its end.

While a course of behavior can be described in different ways, the first description, which is the basis for all others, is of a 'common-sense' variety. Whatever its complications may be, we must always start with a concrete situation; such, for instance, as hunger and the presence of the mother's breast. This situation is followed by the rhythmical behavior of sucking and swallowing milk. The various features of the situation and its attendant behavior can then be described as a perception with its attendant feelings and sensations; but we are never justified in destroying the pattern of the action by reducing it to an aggregation of 'elements.' Other features of this behavior will suggest a biological treatment which will enable us to study the nature of sensitivity, the organic state of the creature, and the articulation of its movements in the act of suckling. But here again the investigator must be careful lest he reduce the behavior to a set of 'simple' acts in mechanical interplay without mutual participation in each other's enterprise. A biological approach to this problem of behavior will also reveal bio-physical and bio-chemical features, which must be examined by the special methods of these sciences. Taking all these features together, one ought to be able to investigate the pattern of behavior by treating each of its special features in the particular way best suited to describe its membership in the complete act.

What the investigator must seek to overcome in the end is any radical difference in the results of these several methods of inquiry. The behavior of nursing is not a set of parallel patterns—psychological, biological, bio-physical, and bio-chemical—although specialists sometimes leave us with that impression. Instead, the common-sense pattern of description, which supplies the problems of each of these sciences, remains the pattern of the behavior itself. However varied the approach may be, each special science describes the one identical act of behavior in its own terms. While these terms are very different in psychology, physics, and biology, the features of the concrete behavior which these terms help to elucidate are only the subordinate particulars of a larger whole, the dynamic pattern of which is one and the same. Thus, not only does the investigator begin with a concrete common-sense description, but he must return to it again and again with whatever enrichment of detail his scientific inquiries have supplied. When we study psychology, or physics, or biology, we do not enter separate worlds; we remain always in the same world. Beginning and ending with the same concrete problem,

we merely supplement each other's observations of one and the same thing. Whatever the method may be, science is always concerned with a single underlying form of being and behaving.

## III. Gradation

Gradation is a way of improving behavior which operates within the processes of differentiation and assimilation. Gradation may be called a universal adjunct of behavior because no differentiation or assimilation ever occurs without some gradation of direction in space or time, in quality or intensity. But there are also situation-responses in which gradation is the most important formative factor. A persistent direction or a rhythmical time-sequence may distinguish an action in which differentiation and assimilation are not clearly marked. Thus a certain inclination may persist in locomotion or in a direction of gaze. Similarly, a certain rhythm may manifest itself in quite diverse movements or in a succession of quite different sounds.

Systematically considered, a *gradient*, as we shall call it, is a feature of a particularized whole. When by differentiation, $A$ is opposed to *not-A*, $A$ may include a variety of gradients, such as right-left, before-after, light-dark, strong-weak, etc. In other words, the particularized $A$ may include a subordinate *a-b-ness*, wherein $a$ and $b$ are notably different although they grade from one to the other in such a way that no boundaries or contours enable us to particularize. A gradient is thus a feature of behavior which, except by the application of an artificial scale of measurement, cannot itself be particularized. Durations, directions, quantitative and qualitative changes, are continuous within the limits of their beginnings and endings.

As previously noted, the form of a particularized whole has its origin in a focus or emphasis that gives it a more highly integrated structure than its surroundings. Gradation is the cardinal principle which supplies the dimensions of the form that is thus integrated. Before integration can be precise, the gradient which determines its structure must begin and end with precision. It is these terminals that describe the contours of the behavior. Although gradation is a feature of particularization which operates within the processes of differentiation and assimilation, it cannot be said to be genetically subsequent to these other two ways of improvement, because primitive differentiations and assimilations depend upon gradients that

have no definite terminals. Consequently the form behavior takes under the circumstances is a gradient and nothing more. If the form in question is a circle, we can illustrate the point by saying that in the beginning a circular form is only a central nucleus which spreads more or less equally in all directions without a precise boundary. A point of light shining through a mist will give us this impression of circularity, although we are unable to define its boundary, or tell just where the light ceases, and darkness begins. Still the impression we receive is clearly that of a circle.

The concept of the gradient has recently been employed in the description of organic growth and behavior. In particular, the systematic researches of Professor C. M. Child[6] have indicated that degrees of metabolic rate, starting in what is defined as the head-region of the animal, control both its organic development and the course of its behavior. Child's use of the concept of gradation is something quite different from the ordinary concept of a mechanism of separate though dynamic parts, functioning together as a result of contact; for, whereas parts are limited, a gradient requires neither a finite beginning nor a finite end. Beginnings and endings there are, but they must first be *achieved* before they can be defined. The establishment of the contour of a form is an *achievement*, prior to which there must be a formal gradient, without sharp limits.

Improvement in learning consists in defining limits, in establishing contours, and in perfecting forms. As this process goes on, differentiations and assimilations take place within the gradient itself. What was a uniform whole becomes a scale of parts. But since each part remains a member of the whole, it contributes to the integration of the whole at the same time that it becomes particularized. Without the differentiation of subordinate parts, the whole would achieve no definite contour; and without the assimilation of these parts, the whole would disintegrate. That is why gradation is the cardinal principle of integration, without which differentiation and assimilation would lack all means of improving behavior.

Whatever is perceived or done has its degree of integration. When the degree is low, the form is vague—little more, perhaps, than an accent in an otherwise unformed continuum. But when the degree of integration is high, there is a scale of parts, each of which is defined in the manner best calculated to fit it into the system of the whole.

[6] C. M. Child, *The Physiological Foundations of Behavior*, 1924.

We can now see why perfection is so rare a thing. The possibilities of definition and integration are so varied that only an arbitrary system, like that employed in some phases of mathematics, will permit of a complete survey. There the simple processes of addition, subtraction, multiplication, and division are clearly defined, and the solution of problems by such means is categorical. But that is because of the self-imposed limitations which these methods employ. Something of the same precision attaches to the proper use of words in sentences and paragraphs, and to the employment of these and other terms in logical deductions. A work of art can also achieve a certain perfection, because the artist is able to control his material and to compose his work by an arbitrary selection so that his product is sure to be articulate.

In less articulate undertakings integration has no fixed bounds. Working outwards from a center, differentiating and assimilating as the conditions warrant and behavior permits, one never can tell when the ultimate perfection of form has been attained, because every slight alteration of persons and conditions sets a new task, requiring a different method of fulfillment. Records made in various sports are constantly being broken. While it would not be difficult to set a limit in any given sport beyond which no human being will ever be able to go, it is quite impossible to say at what precise point a record will be achieved which can never afterwards be broken. Apart from arbitrary schemes, which are a product of man's genius for abstraction, there are no precise limits in nature, but only gradations.

## IV. Redefinition

Redefinition is the fourth and last way of improvement in learning. Practically speaking, redefinition means versatility. From what has previously been said of formation by differentiation, assimilation, and gradation, we might suppose that forms are infinite in number and variety. On the contrary, forms constitute a hierarchy of kinds in accordance with which one and the same form may serve in different contexts as different things.

Perhaps the first reaction to this proposition is that of scepticism. How can one and the same form be different at different times and in different places? If the form has been precisely defined this of course would be impossible. But when we bear in mind that form is a growth, the contours of which though essential to its complete definition are never absolute or precise save in an arbitrary system,

we can see how the same nucleus or focus with its gradients can undergo a material change of contour without surrendering its fundamental identity. Think of the perception of circularity and the infinite variety of round things to which this perception applies. These are all redefinitions of a fundamental form perceived in a variety of different objects, functioning in a variety of different contexts. Yet so strong is the impression of the precise form of roundness, that we are able to grasp it even under unfavorable conditions; for instance, when we see a plate as being round although its perspective makes it elliptical.

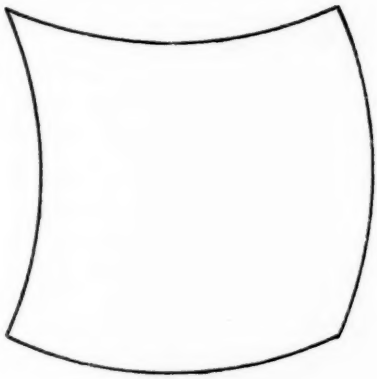

FIG. 1

The vagueness of certain other forms which have a low degree of integration makes them serviceable in different ways. Thus the principles of behavior in lower organisms are few in number, and the animalcule seems to be able to do all that it must do in order to live—that is, to move about, to find food, to eat, to digest, to excrete, and to reproduce its kind—with a marked similarity of method. At a higher level of integration some patterns of behavior become stereotyped. Though lacking the variability of primitive gradients, these stereotyped forms of behavior are still modifiable in varying contexts. One does not grasp a spoon as one grasps a pencil, yet something of the same pattern of behavior is involved in each action.

Occasionally a form will change its internal structure while retaining its precise contour. Consider the adjoining figure which can be perceived in three different ways: as a flat 'square' with curved

sides, as a curved surface, and as a shield, the axis of which runs from the upper left to the lower right corner. Strikingly different as these forms are, the same contour is adequate for each. Phenomena such as these are not at all primitive. If we did not already know the typical forms of plane and curved surfaces, and of shields, it would be hard to perceive such different things in one and the same figure.

The point is that the precision which stamps a certain form as invariable depends upon a certain context. In some other context the same outline may constitute a different form. But context is not always determined by experience; it may depend upon conditions which are operative without previous process of learning. Thus, redefinition may occur as a result of shifting forces in the perceiving organism, especially in its central nervous system. On this point Pavlov writes: "Certain experiments, if confirmed, will have demonstrated that such an important cortical function as synthesis ('association') may take place even in those cortical areas which are in a state of inhibition on account of the existence at that moment of a predominant focus of strong excitation. Though the actual synthesizing activity may not enter our field of consciousness, the synthesis may nevertheless take place, and under favorable conditions it may enter the field of consciousness as a link already formed, seeming to originate spontaneously."[7] In addition to 'cortical synthesis' there is also the possibility of a redistribution of weight and balance in the phenomenal field of perception, which will account for a new context without a preliminary process of learning.

The outline of our figure happens to be equivocal. Its redefinition as different forms is therefore in large measure artificial and disingenuous. But so are many other educational results where redefinition occurs only because a certain definite contour happens to have different contexts in experience. And there are still other instances where redefinition appears to be spontaneous and never before experienced.

Redefinition is therefore a fourth means of improving behavior. In a given situation, a genius will see many things where an average person sees but one. Indeed, it is a mark of genius to be able to perceive the various distinct possibilities of an equivocal situation, which to the average person is only confusing or ambiguous. But there is a difference between the art of dealing with equivocal situations and the ability to grasp fundamental issues. The first is what

[7] I. P. Pavlov, *Conditioned Reflexes*, tr. 1927, 410.

we call *cleverness* while the second requires *insight*. Redefinition accounts for improvement of both sorts. A clever person makes the best of superficial appearances, while a thoughtful person seeks to redefine a situation with reference to its basic pattern.

The question of basic patterns constitutes the central problem of behavior, both as regards the process of learning and what is learned. We have already said that forms are not infinite in number and variety, but instead that forms constitute a hierarchy of kinds. The criterion of these fundamental kinds of form is the fact that they are *outstanding*. Among a group of slightly variant forms we recognize one as being the outstanding type to which all the others belong. This recognition of outstanding types of figure and process, which we call *insight*, accounts for all scientific principles and postulates. With perception as our point of departure, we proceed to study these outstanding types and their variants. We may do so in terms of receptor-processes and effector-processes; or we may do so in terms of physically conditioned and psychologically revealed phenomena. When investigated under varied conditions in these various ways, the form studied no longer appears to be the same thing. Yet, the basic pattern with which we began our inquiry does not change. Thus, for instance, *circularity* as a basic pattern belongs to the moon as a physical object, as a perceived disk of light, and as a retinal impression. These three are not the 'same' form, yet each of them has the formal quality of circularity. This common feature of identity has a profound significance, because it enables us to bridge the gap which would otherwise intervene between the moon as a physical object, its psychological perception, and the physiological processes whereby it is perceived.[8]

## V. Reality and Participation

Differentiation, assimilation, gradation, and redefinition exhaust the possibilities of improving behavior. They have also a wider significance. Being equally applicable to physical, to biological, and to mental processes, they constitute the fundamental principles of every existential formation, and are therefore the *laws of reality*. For instance, the atom of physics is a differentiated whole, the members of which—protons and electrons—are the features of its dynamic

[8] Cf. the author's Crossing 'the Rubicon between mechanism and life,' *J. Philos.*, 22, 1925, 281 ff.

pattern. Any loss or gain of electrons in such a system is analogous to the rearrangement of our figure, which transforms a square with curved sides into a shield. The mathematical physicist finds it convenient to assume that protons and electrons are particles bearing electrical charges which determine their positions and orbital movements in a neutral medium. But this medium is only a background for the formal manifestation of the atom. In reality we are dealing with a pattern, the electronic moments of which are its features, like the waves upon a sheet of water which rise and fall without translating liquid particles from one place to another. In so far as an electron possesses reality in its own right, it is a form upon a neutral or unformed ground. When an electron appears to pass from one orbit to another without occupying the intervening space, we are confronted with something inconceivable so long as we persist in regarding the electron as a distinct and self-contained unit of matter. But if we take the electron to be only an accent of electronic force, this conceptual difficulty of translation disappears. What we suggest is that the very existence of the electron as a unit is conditioned by the system in which it occurs, and that when the system is altered the electron can either vanish altogether, or it can reappear as an accent at some other place in the system.

Our theory leads us to conclude that entities, contents, materials of any and all kinds are in reality conditioned by the systems in which they occur. The essential reality of any system is form. Despite its apparent concreteness, material is in reality a creation of form. Consequently any seemingly stable material thing, like a grain of sand, or an atom, or an electron, exists only because, and so long as, the system in which it belongs permits.

The physical universe, taken in its entirety, is such a system, and its material parts—the stars, the earth, organic life, man and his mind—all find their respective 'places' and 'times' in accordance with the principles of differentiation, assimilation, gradation, and redefinition. These same principles of formation reveal themselves whenever we penetrate into the structure of things, living or inert, just as they do when we examine into the processes of mind.

If we chip a piece from a smooth round pebble, the pebble will not reconstitute its smoothness and roundness. The destruction of its form has no appreciable effect upon the remainder of the pebble, and what remains will continue to exist without obvious change until its roughness has been smoothed by some external means. But when

a living creature is maimed it either dies, and its physical form disintegrates, or, if it lives, it must quickly compensate for its maimed condition by regeneration and readaptation. The pebble finds its place, and plays its part in the order of things, chiefly with reference to external conditions. Thus the manner in which it occupies one place or another is highly contingent; and so too is the manner in which it preserves its shape. The pebble may be an aggregation of crystals, in which case each crystal has an inherent form. Yet the existence of the pebble as a whole is less cohesive than the existence of a living creature, or, indeed, the existence of those atoms which constitute the molecular structure of the pebble's crystals.

In order to determine the formal existence of inert matter, we must either go back to its atomic structure or forward to its geological position in space-time. The aggregate existence of a pebble, which may roll into the water or be washed back upon the shore, is highly contingent. That is to say, its internal crystalline form cannot guarantee its continuance as a smooth, round pebble, or its position on land or in the sea.

In the case of a living plant or animal, however, the informing principles which we have described operate in addition to the contingencies of aggregation. While the atomic structure of living tissue is presumably of the same order as any other atomic structure, the form of the organism as a whole is capable of self-differentiation, assimilation, gradation, and redefinition. These ways of self-improvement are not shared by inert matter. The growth of a crystal has, to be sure, some of the characteristics of the growth of a living form; and the final distinction between living and inert forms is probably less radical than we commonly suppose. Yet the growth of an organism involves a permeability of influences, both internal and external, which the growth of a crystal lacks. Beginning with a single fertilized cell, development by partition leads not only to an aggregation of cells, but to a coherently integrated whole. The separate cells of a living creature participate with one another in such a way as to establish a gradient of behavior which traverses the entire aggregation of protoplasm.

But that is not all; for an organism is something more than a self-contained, self-regulating unity—it is also a system open at many points to the influence of its inorganic surroundings. Participating thus with its surroundings, an organism forms dynamic patterns of situation-response by selecting those lights, sounds, odors, tastes,

and contacts which become the characteristic features of its behavior. This openness of the system of organic life is somehow different from the closed systems of inert physical forms; for participation with its surroundings enables an organism to bridge the gap that keeps one crystal apart from another, and one pebble from another.

By differentiation units are formed which are more or less self-contained and independent of their surroundings. This independence, however, is very different in its expressions. An organism may be 'free' to move, to approach or retreat, to be active or quiescent. In a word, it is free both to participate in its environment and to shape its own internal arrangements. A pebble, on the other hand, is 'free' in quite another way. The pebble does not readily participate in its surroundings, nor do the particles that constitute its molar structure participate freely with each other. The freedom of a pebble is thus freedom *from* participation—the independence of isolation.

Mind also reveals these two kinds of independence. The freedom of thought is participative. The formation of ideas is contingent to so slight an extent that ideas always purport to be true counterparts of the behavior which they reflect. But when these same mental contents are subjected to analysis they are found to be highly contingent. Thus the elementary data of sensation and feeling have freedom only in their exclusive separateness.

A strictly analytic investigation which separates mind from matter will proceed to classify the contents of each realm as so many free and independent entities. A strictly synthetic investigation, on the other hand, will trace the reciprocal participation of all things in the universe, and will find that these things are not the component parts of the universe, because everything owes its existence to the universe as a whole. Synthetically, the independence of things is relative, and things themselves are in reality functions and not entities at all.

We appear to have two methods of investigation, which lead to two different concepts of freedom. According to the analytic method existential factors are independent and self-contained. When contacts are made, these factors interact because each has a self-determined inertia or disposition to continue in its state of rest or motion. The synthetic method finds existential factors to be independent, but not self-contained; because they are not true entities, but only the shifting features of a formal pattern. The nature of these features determines their relative separateness and self-determination as 'things,' and also the character of the pattern in which they function.

Every method of investigation must be based on some set of assumptions, but as the method is followed, these assumptions will suffer modification in accordance with the revelations of the subject-matter under investigation. The modifications must not be radical, however, for, if they are, the method itself will break down, and some other method based upon another set of assumptions will have to be devised to take its place. The question before us is: Which of these two methods, the analytic or the synthetic, can give us the truest picture of reality—which of them will best account for the process of learning, and at the same time give it its proper place in the order of nature?

## VI. Two Methods of Investigation, and Two Kinds of Freedom

We are considering two methods of inquiry based upon radically different assumptions. According to one set of assumptions, the basic data of reality are self-contained, independent, entities which react with one another, and thus compose aggregations which, in turn, are more or less self-contained, and more or less independent. From the lowest ultra-microscopic entity, such as the proton of an atomic structure, to the highest macroscopic unit of celestial mechanics, everything is assumed to be composed of an aggregation of subordinate things, each operating under its own impulsion with its own degrees of freedom. Life and mind, involving all possibilities of educational change, must therefore be capable of reduction to an interaction of entities which have no material connection. In order to control the course taken by a living being, conditions must first be set to govern the interaction of the entities involved.

The first instance effecting such a control is known as a 'fortunate variation,' which implies both chance and what we call an *achievement*. But what is an achievement amid interacting entities? According to the assumptions of a strictly analytic method one thing can be no 'better' than another; and that things should work well or ill is altogether contingent. Of two pebbles rolling down a slope, one may reach the bottom while the other lodges in a tuft of grass. It does the one no more good to reach the bottom of the slope than it does the other to find lodgment on the way. Of two animalcules swimming about in the water, one finds food and is nourished while the other finds none and is starved. In accordance with the assumption of sheer aggregation which underlies the analytic method, it

does the one no more good to live than it does the other to die; for each does only what it must do under the circumstances. And yet the living, continuing existence of the animalcule that finds nourishment is somehow successful, whereas the starvation that the other suffers is a failure.

But is there an essential difference in these two results? Analytically, no; for success and failure are terms that have no analytic justification. How comes it, then, that we not only value life but seek to control its conditions so as to further its progress? If the selection of a 'fortunate variation' is contingent upon repetition, 'fortunate variations' must occur often; otherwise no improved method of behavior would ever be selected. Experimental observation shows, however, that a 'fortunate variation' can impress itself at once as a fitting mode of behavior which will be ever afterwards retained T o an ape, which had learned to secure fruit lying at a distance from the bars of its cage by the aid of a stick, were given two hollow, bamboo canes. Neither of these was long enough to reach the fruit, but they could be fitted together to make a stick of the required length. Without knowing how to splice them, the ape tried in vain to reach the fruit by using the sticks both separately and together. He occasionally even placed them end on end, as if he did see the need of increasing the length of the stick. On one occasion the smaller stick slipped into the hollow core of the larger one. Immediately the ape made off for the fruit with his now lengthened stick; and although it fell apart before he could use it, he was at no loss to fit it together again, this time firmly enough to serve his purpose.[9]

Here we have a successful achievement following a 'fortunate variation.' But in the case of this particular animal no repetitions were necessary in order to learn this act of splicing; for on the succeeding days the ape proceeded at once to splice the sticks without preliminary variations, fortunate or otherwise. Apparently there is something more to a 'fortunate variation' than mere chance; it possesses an inherent quality of fitness which other variations lack. We cannot say that two sticks are any 'better' when they are spliced than when they are separate; but they are a better tool for reaching distant objects. Indeed, we have here an improvement in quality which rests on the fact that two sticks have been transformed into one.

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[9]Cf. W. Köhler, *The Mentality of Apes*, tr. 2nd. ed. 1927, 125 ff.

In accounting for such instances a strictly analytic method breaks down. If we attach any significance at all to the *achievements* of living creatures, we shall have to devise a different set of assumptions for our explanation, and follow a different method of investigation from that of strict analysis. So long as we are dealing with pebbles rolling fortuitously down a slope, the value attaching to one that reaches the bottom in comparison with another which is hindered by a tuft of grass is quite extraneous. The pebbles themselves need suffer no considerable material change wherever they land. In some respects the same is true of an animalcule that thrives in comparison with another that starves; for the success of the first and failure of the second are alike actual processes. Nevertheless, in the case of animals the first of these processes has a quality which the second lacks. The function of prolonged life is the persistence of certain forms of behavior, whereas death is the destruction of these forms. While in death other forms replace those of life, these other forms are no longer 'successful' in the sense in which prolonged life is.

We must alter our assumptions in order to make a place for success. We must do this, not merely in the interest of a special kind of achievement which analysis is unable to account for, but also in the interest of a completer survey of the facts than analysis alone is able to give.

The method of analysis breaks down because it can give no adequate insight into the order of things. Without order there could be no uniformity, no regulation, nor even those assumptions upon which the methods of analysis themselves are based. The method of synthetic formation, on the other hand, assumes order and proceeds to investigate the appearance and disappearance of those factors which grade, differentiate, assimilate and redefine themselves so as to create the things we know and are concerned about.

In the course of these various and overlapping processes which make things what they are, aggregations occur; for not everything is completely and successfully formed in its own right. Some things are self-contained and independent in their immediate context, whereas other things participate freely with their surroundings. A pebble owes its form to remote and contingent circumstances, and its share in what goes on round about it is very slight. A living creature, on the other hand, is both self-regulatory and participative, and its share in its surroundings can be very large.

Thus there appear to be two kinds of freedom. One is a freedom of immunity from the immediate influence of surroundings. This is the freedom of a stone, and it is also the freedom of a man when he exercises mental reserve or aloofness. In both cases deep-lying and remote influences are largely, though perhaps not completely, held in abeyance. The dynamic form of the atomic structure of a pebble is locked up, as it were, in the aggregation of its molecules. So too, the geologic and celestial influences of world-formation are incredibly slow and remote in their effects upon this same pebble. Man's reserve, or aloofness, on the other hand, is only a transient phase of his life; for in addition to his atomic structure and his place as a molar entity in the world-system, man is no mere aggregation of parts, but a flexible unified whole, sensitive both to his surroundings and to his own internal structure. In order to be able to stand aloof and refuse participation with his surroundings, man requires a degree of internal cohesiveness very different from the inflexible inertia of a stone, or even the more flexible inertia of a low form of life. Primitive forms of life have no such reserve; even an animal's state of rest and quiet is less an inhibition than an equalization of impulses. In other words, the static condition of a stone is a natural contingency, while the static condition of a reserved mind is a highly artificial acquisition. A stone does not learn to be reserved, whereas a man does.

The second kind of freedom is participative. Here the influences are mutual and shared. This is no freedom *from* other things, but a freedom *with* other things. It occurs everywhere throughout the universe, but it has three notable manifestations: (1) in sub-atomic structure, (2) in celestial mechanics, and (3) in life.

The structure of an atom, as modern research reveals it to us, is an electronic mutuality which can be described as a pattern of interrelating forces. A similar description is appropriate for celestial mechanics and the formation of world-systems. Between these two extremes all things find their places with higher or lower degrees of internal and external mutuality. A pebble, as we have seen, shows but little of this quality, since the developing forms of its atomic sub-structure are locked in the dead embrace of its aggregation of molecules. As for the influences exerted upon it from without by gravity, friction, and propulsion, these are contingent, and largely unshared by its molar mass.

Many other physical objects can be found which show greater degrees of mutuality. The magnetization of iron, the flow of currents, liquid or gaseous, electrical fields, radiations, all are mutual and participative. But it is the living organism which shows the quality of mutuality in its highest form. Self-regulation and participation are the principals of life, and the loss of these principals is death. While not different in kind from the freedom of an electron or a comet, the freedom of a living creature is less contingent than either of these. The self-regulatory changes which take place even in lower organisms are selectively appropriate to changes in the animal's environment; and these two sets of changes evince a mutual interdependence which is more obviously participative than it is with either electrons or comets.

Being thus capable of self-regulation, and of an activity in tune with its surroundings, an organism can be *educated*, whereas an electron or a comet cannot. By this same token, education *is* self-regulation, and is founded neither upon coercion nor upon remote influences.

But while education takes place as a natural consequence of organic life, we should not make its definition so broad that it becomes a useless synonym for life itself. The restrictive statement that "education is a social process of change in the behavior of living organisms"[10] will better serve our purpose, emphasizing as it does the behavior of the entire organism, especially in its social relations. Education is concerned neither with the remote influences of atomic structure and world-systems, nor with the vegetative process of sheer living as a participative function of organic economy. It is concerned with the relations of one being to another, and with the progressive solution of problems both great and small that confront the individual and the group. These solutions can all be regarded as improved ways of doing things, and their methods are ways of formation which can be systematically treated as differentiations, assimilations, gradations, and redefinitions.

Learning defined as improvement furnishes the key to a philosophy of education which finds its values inherent in reality itself. Once we have overcome the bias of a strictly analytic method of investigation, and are ready to accept form as being more real than substance, we can envisage the universe as a progressive system of

[10]Cf. the author's *Psychology and Education*, 1926, 5.

events, the law and order of which depend neither upon an elaborate mechanism of free-moving particles, nor upon an idealistic teleology which somehow shapes the destiny of things.

But until this bias has been overcome, until we can realize that matter, life, and mind all participate in the one great adventure of existence, we shall continue to be baffled by the conflict of materialism and spiritualism which up to the present has been the main theme in our history of philosophy.

# A NOTE ON RECOGNITION

By E. Murray, University of Illinois

In any attempt to systematize and to explain the variable factors of experience, one process inevitably taxes the ingenuity of the psychologist—the instant, arresting flash of intimacy or *knowness* attaching now to a voice, now to a perfume, to a face in the crowd or to a bar of music. Direct recognition of this type, *i.e.* recognition unmediated either by an explicit reference or dating or by definite verbal associates, remains resistent to exact analysis and description when all else yields; the storm center in many a clash of opposing theories and systems.

To accommodate this refractory process psychologists were at first inclined to enlarge their repertory of terms, positing an unique *Bekanntheitsqualität*, a kind of 'deepening' of consciousness, referred to the heightened susceptibility of the cortex to a second stimulation, or (more specifically) to a blending of the experience in hand with a *revived image* (Höffding, 1889). Experiments in the Wundtian laboratory, however, demonstrated the difficulties in the way of recognition of simple perceptual material and cast a doubt on this construction (Wolfe and Lehmann, 1886 and 1889); and the observations of Bentley, Whipple, and Gamble upon grays, tones, and odors, further discredited the notion of a direct, invariable dependence of recognition on the memory image. Speculation took a new turn and the 'penumbra' theory, earlier advocated by Külpe and by James, came into vogue.

The baffling face or melody (it was argued), which tantalizes us with our impotence to supply name or context, represents an abortive case of 'explicit' recognition. Since the latter is commonly mediated or accompanied by a host of centrally excited associates, the familiarity tone which attends direct recognition is presumably an aura of checked, half-realized associative processes; *i.e.* nascent images, incipient motor adjustments (tentative whistling of an air, sub-vocal verbal articulation), or the partial reinstatement of bodily set or mood. Wundt's *Auslösungsgefühl*, Titchener's weakened survival of the emotion of relief (both vitiated by their arbitrary exclusion of cases of unpleasant, indifferent or exciting recognition),

Washburn's earlier hypothesis of a unique process correlated with ineffective cortical discharge,[1] and the *Einstellung* of Betz[2] may all be subsumed under the 'aura' caption.

As for the factor of promptly reinstated motor set or habituated response (the 'habit' theory of recognition), as illustrated by the identification of a lost key or pen through the ease with which it settles into one's fingers, much of the experimental work of the last ten or twelve years (*e.g.* Clarke,[3] Woods,[4] Owen,[5] and Strong[6]) emphasizes its importance, at least in certain forms of recognition and with certain types of material. It may be objected, however, that automatized or implicit recognition (*e.g.* the mere 'of courseness' attaching to the hundred and one objects of one's everyday surroundings) has little in common with the function now before us, the instant detachment of the familiar from the unfamiliar on the basis, it may be, of a single prior contact.

Baffled by the elusive nature both of direct recognition and of the 'sense of familiarity' which attends all memorial material, many psychologists have fallen back upon an intellectualist (not to say obscurantist) position, pigeonholing recognition as an irreducible 'imageless process' or an elemental act of consciousness or of the self (*e.g.* Calkins, McDougall, Dürr, and others of the Würzburg school).

Reports from a large number of subjects who checked a sheet of misspelled words throw light upon the essential nature of unfamiliarity or strangeness. The misspelled words in the text arrest the observer's progress over the sheet, much as the tower landmark in a strange city arrests one's gaze; but with a difference. Within the limits of the word itself the gaze is restless, unpivoted, and the word breaks up into fragments.[7] The incipient articulatory kinaesthesis aroused by the correctly spelled portions of the text, normally fused

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[1]M. F. Washburn, The process of recognition, *Phil. Rev.*, 6, 1897, 267.

[2]W. Betz, Vorstellung und Einstellung: I. Wiedererkennen, *Arch. f. d. ges. Psychol.*, 17, 1910, 266 f.

[3]H. M. Clarke, Conscious attitudes, *Amer. J. Psychol.*, 22, 1911, 214 f.; A note on recognition, *Stud. in Psychol., Titchener Commemorative Volume*, 1917, 175 f.

[4]E. L. Woods, Experimental analysis of the process of recognition, *Amer. J. Psychol.*, 26, 1915, 313 f.

[5]R. B. Owen, Recognition, a logical and experimental study, *Psychol. Monog.*, 20, 1915, no. 86.

[6]M. H. and E. K. Strong, The nature of recognition memory and of the localization of recognitions, *Amer. J. Psychol.*, 27, 1916, 341 f.

[7]For an analogous phenomenon see E. Severance and M. F. Washburn, The loss of associative power in words after long fixation, *Amer. J. Psychol.*, 18, 1907, 182-6. As different portions of the word in turn obtrude themselves, the feeling of familiarity disappears, replaced by a sense of unfamiliarity or strangeness.

with the visual impression, is replaced by clumsy, self-conscious or self-referred efforts of tongue and lips and throat to grapple with combinations for which previous experience often provides no precedents. Delay and incoördinated articulatory efforts (sometimes laughter) replace the smooth-flowing, habituated adjustments of reading.

Analogous features mark the commentary of the subject on the differentiation of the familiar and the unfamiliar in experimental series made up of postcards, advertisements, and photographs. "I remember the nose," "the tree in the background," "the color of the car," declares the observer, adding usually on interrogation that there was no actual repercussion of the memory image, no overt comparison, no recall of a name or descriptive term; but merely an automatic arrest of the gaze on the feature in question, a facile exploratory movement of the eyes over a profile (in the case of a Holbein portrait), an involuntary mimesis of posture or expression (with photographs of cinema actors), or an empathic kinaesthesis (in the case of a tree, vase, or Gothic arch). A feeling of aversion or repugnance attaching to the percept recognized, or a thrill or glow of satisfaction appropriate to the content, or provoked by the successful carrying through of the act of recognition (as set by the conditions of the experiment), were also occasionally noted.

In the case of the unfamiliar or unrecognized, on the other hand, there is no single motor set, no relatively stable innervation, and no kinaesthetic imaging. Instead, the roving, questing gaze of the traveler in strange surroundings, a restless shifting of items in attention, according to the temperament and set of the subject and the conditions of the experiment. The overlay of strangeness in dream content, as reported by the majority of dreamers, is doubtless correlated with a similar texture or pattern. Motor phenomena customarily drop out or are in abeyance; but the relative inconstancy or instability of the imagery simulates the effect of a wandering gaze, and the dreamer drifts through an endless World's Fair, Garden of the Gods, glorified apartment store, or what not, never able to stay or retrace his steps or to retrieve an article dropped or forgotten.

While familiarity, according to the formula here adopted, is thus a matter of pivoting on one of several items, a discursive play of eye movements or of kinaesthetic imagery over the subordinated features of the percept undoubtedly occurs; though with invariable return and momentary pause on the primary item, thus throwing in relief,

through contrast, the relative stability of the pivotal feature. The facility of recognition where more complex, meaningful material is involved becomes therefore readily intelligible without recourse to extraneous or doubtful factors, such as the wealth of associative processes assumed by the devotees of Herbart. Neither is reference to a hypothetical faculty of attention necessary to support our view. The efficacy of the kinaesthesis accompanying binocular fixation (over and above the asset of clean-cut foveal vision thus automatically secured) has more than once been demonstrated.[8] An analogous power to reënforce or arrest elusive processes possibly resides in the auditory mechanism of the middle ear, or in the kinaesthesis that accompanies its functioning.

A single vivid experience in which central set or chance associates bring into special prominence some one feature—color, a fragment of outline, rise and fall of pitch, accent, or timbre—is sufficient to motivate fixation or empathy, on recurrence, and to pivot perception on this feature. Well-defined cases of direct, truncated recognition appear of course only before automatization of a percept has gone very far, and preferably against a background of the unfamiliar.

This formulation of the essential conditions underlying the 'familiar' as *centering and arrest* has obviously much in common with Washburn's motor theory of attention, in terms of which it might readily be recast. It is, moreover, susceptible of application to a wider range of experiences than the familiarity moment incorporated in *Movement and Mental Imagery* (1916, 203). The latter leans too heavily upon the *Auslösungsgefühl* of Wundt and the 'feeling of relief' of Titchener, whereas experimentation in recent years points to the presence only of specific feelings, appropriate to the recognized percept, and variable with the task and the conditions of the experiment, and with the set and temperament of the observer, whether passive or adventuresome, whether craving placidity or change of state.

Along with the features of the total recognitory pattern, as sketched above, certain of the much-stressed factors of the earlier hypotheses may play a subordinate rôle. The momentary arrest in perceptual experience of a given item or attribute may be in part conditioned by earlier cortical excitations, which operate to enhance the *Aufdringlichkeit* of the present impress, even though no memorial

[8] E. Murray, Peripheral and central factors in memory images of form and color, *Amer. J. Psychol.*, 27, 1916, 241.

imagery is distinguishable; delaying the passage of the item by increasing its effectiveness as an incentive to fixation. In the case of complex visual or auditory percepts, inklings of fragmentary, bodiless imagery may actually forestall or adumbrate the successive moments or movements, conveying hardly more than a sense of a quickly vanishing doubled outline. Especially is this true in following through a familiar air, where every turn of the melody seems to fit into one's *Bewusstseinslage* as a hand into a glove. The peculiar phenomenon of paramnesia or *fausse reconnaissance* is apparently an experience cut on a similar pattern but abnormally conditioned. A momentary block or gap in the perceptual function, due to 'nerve fatigue,' dissociates the two stages or phases; resumption of function stamps the earlier anticipatory portions as a remote prior experience —the re-coalescence of the two provoking a vivid but arrested recognitory awareness.

In conclusion, this view of recognition as essentially a pivoting of perceptual contents (reënforced by ocular fixation, exploratory eye-movements, mimesis or empathy; by motor adjustments prompt, accurate, effortless, projected or read into the pivotal feature), which connotes a momentary stability in time, places this experience between cognition, on the one hand, and aesthetic perception, on the other. In cognition, or perception with general reference, there is also pivoting, emphasis, but upon the features common to the class, rather than upon the casual items which practical experience has rendered salient. In the aesthetic perception, on the other hand, there is an artificial centering, controlled by the provisions of the artist, who disposes his materials in such fashion as to provoke certain exploratory or mimetic pivotal movements. Out from the center thus established there run diverting detours, providing novelty and variety without disintegration or decentralization, as in the Spring of Botticelli, the Meyer Madonna by Holbein, or the first movement of the Heroic Symphony by Beethoven. Except in the case of totally novel art-forms, this skilful arrangement of details forestalls the prior experience necessary to humdrum recognition, affording at first encounter an alluring sense both of at-homeness and of adventure.

# FORCED ADJUSTMENT *VS.* ASSOCIATION IN CONSTRUCTIVE LEARNING AND THINKING

By JOSEPH PETERSON, George Peabody College

The doctrine of associationism originated and was developed out of logical formulations of the play of 'ideas' when almost nothing was known of neural elements and their inter-relations, and when the problems of constructive learning—learning involving elimination of 'wrong' responses—did not exist. Yet it has survived, with only slight modifications, into a period of new experimental investigations, in spite of the fact that every experimental test applied to its basic principles has found them inadequate and even positively wrong as applied to constructive learning. Notwithstanding these difficulties and others that have arisen in connection with our newer knowledge of the nature of the nerve impulse—refractory period, all-or-none law, etc.—and the processes of inhibition, many writers still look for some undiscovered means of making associationism, possibly in some new dress, serve all our needs so far as explanation is concerned. The modern 'conditioned reflex' enthusiasts are still in very close agreement with the general position of David Hartley and James Mill. They of course base their doctrine today on certain new physiological developments, but disregard many other developments as well as experiments in psychology which contradict their position; and they attempt by mere reiterated assertions, even contrary to some of their own experimental results,[1] to establish the original view of the all-sufficiency of the principle of contiguity.

In earlier days associationism met no difficulties in psychology, because wherever the associative principles failed they were buttressed by arbitrary faculties of some sort—thoughts, perception of ends, attention, will, judgment, intelligence, etc.—and the real problem was unwittingly concealed. New adjustments and departures from expectations based upon frequency and recency factors presented no problems; they were simply the results of interference by some faculty, entertained unsuspectingly, of course, under some other

[1]Cf. Kuo's experiment cited later, and the failure of the conditioned reflex to strengthen and perpetuate itself.

name. Most writers would hesitate to assume faculties outright, because science leaves no place for arbitrary forces, and their existence has never been proved.

It would of course be extreme to deny any place at all to association principles in present-day psychology. Many simple connections in our thinking and general behavior—including the functioning of glands—are doubtless best accounted for on the familiar principles of James' elementary neural law of contiguity. However, it must be frankly acknowledged that few established facts, even in fields where associationism has worked best, agree closely with the doctrine in important details.

Take, for instance, the facts of purely reproductive learning. In these cases the *S* is so 'instructed' and limited by the conditions of the experiment, which are usually overlooked in theory, that to get out of the laboratory-situation he must finally give the responses required in the order demanded. Any detailed investigation of all these responses will reveal numerous irrelevant and even positively wrong reactions; wrong responses (words) are given; many that are correct occur out of order; and omissions between rightly reproduced elements occur in ways wholly unaccountable on the basis of the doctrine in question. In orthodox circles these are either overlooked or explained away until each of the association laws—frequency, recency, vividness, and primacy—gets its due allowance. Divergences from expectations give little trouble in view of the numerous disturbing conditions on the basis of which any irregularity is readily explained. How easy it is to see something that is looked for, especially when it is the sole thing expected! In the experiment the *S* is supposed to be wholly under the spell of the present situation including the instructions. Little note is taken of the situations in which the meanings of the several dictated words have developed and of the attitudes they arouse, of *S*'s present organic disequilibria, and of troublesome situations from which he has recently come. Of course, *S* is asked some questions about these situations and about bodily conditions, but the replies—as well as the troublesome erroneous responses above referred to—do not fit into the general scheme of classification and are therefore usually not well incorporated in the general body of results.

In the more purely reproductive memory, or recall, association has always been the sole operating principle to most of those who have rejected faculties like that of 'memory,' and this has been a rich

field for the supply of illustrative material by the associationists. Even James, keen observer that he was, held rightly enough that, "mental spontaneity . . . can certainly not create ideas or summon them *ex abrupto*," and he maintained that it can only wait on the associative principles to bring them up. When once they come down the 'stream of ideas,' however, 'interested attention' or 'volition' might divert the stream by somehow mysteriously emphasizing or protracting an idea even if only for a second.[2] Though he saw no choice but that between association and 'spiritual spontaneity,' he was too accurate a student of the facts of life to rest content with mere association. While he included 'spiritual force' also he treated it mainly in footnotes, because, in his own words, it 'interfered with the smoothness of the tale;' it was unscientific. It is true that for Bain, James, Stout, and many others, association by similarity, of which contrast was but a special case, made serious inroads on the pure principles of association; it foreshadowed the rôle of the 'perception of ends,' of 'meaning,' *Gestalten*, motives, etc.; in more objective and recent terminology, of 'sets,' 'attitudes,' 'determining tendencies,' 'drives,' etc. In the writings of all these authors, however, faculties under various other names played leading rôles in the higher mental functions.

Binet also found mere association inadequate to explain recollection. He and Henri asserted that "those who take the pains to make observation of facts will perceive that the principles of association do not account for all the phenomena—far from that; and we propose to show, *à propos* of our experiments on verbal memory, the existence of an entirely different mental operation which is explained neither by contiguity nor by resemblance."[3] They found, as they interpret it, that recall may be accomplished by sheer effort with apparently no association. Unfortunately they attributed the effort or tension only to a directing of 'attention,' or to 'an idea of the whole.' Though Wundt early came under the influence of James Mill's *Logic* he departed very far from the principle of associationism employing such conceptions as *Verschmelzung* and apperception.[4] Texts in experimental psychology have usually included experiments on retention and reproduction to demonstrate the effects of repeating words

[2]William James, *Principles of Psychology*, 1890, I, 594.
[3]A. Binet, and V. Henri, La mémoire des mots, *L'année psychol.*, 1, 1895, 21.
[4]Cf. E. B. Titchener, Wilhelm Wundt, *Amer. J. Psychol.*, 32, 1921, 161 ff.; see especially 164 ff.

(effect of frequency), of inversions and heavy printing (vividness), of early and late position of members (primacy and recency) in a series. The problems of learning were in the main confined to verbatim fixation in memory, or reproduction.

The first real challenge of association laws on a purely experimental basis in a situation where they might, if they were as effective as alleged, have fully justified themselves, was probably an experiment I carried out in the summer of 1916 in the University of Chicago laboratory.[5] Rats, fortunately knowing nothing of the laws in question and being free from any limiting instructions, were put into so simple a situation—a maze—that they, through a series of 'choices,' had simply to take either the right or the left course at each bifurcation. They were motivated by food drives and were left in the maze in each trial till the food box was entered. Every choice and return run in the maze by each of 17 animals was carefully recorded as to its position in the maze. The situation was thus made relatively simple and the 'laws' were left to speak for themselves. If, for example, at any bifurcation of the 4-in. alley the rat had gone four times and most recently to the left, and had made but three or fewer runs to the right, the laws of association would, of course, except for special circumstances, make him take the left route again. Well, not knowing the laws, he and all his 16 fellow rats failed to obey them. The *E* had only to count up all such situations—1232 of them for the 17 rats—calculate the expectations at each one, *i.e.* predictions on the laws of recency and frequency (vividness and primacy being controlled), and tabulate the actual choices made by the animals, who did not even suspect that they were testing out any 'laws.' When the results were tabulated and compared with predictions it was found (1) that the 'choices' of the rats violated frequency and recency laws nearly as often as they followed them; (2) that agreements between the choices and the laws occurred no more frequently in right acts (choices making toward the food) than in wrong ones; and (3) that the frequency and recency factors in consideration showed no discoverable relation to learning of the sort studied. In many cases the stereotyping effects of these factors was obvious, but the factors seemed to fix wrong as well as right acts. Indeed, learning often seemed to demand a direct breaking away from the effects of these factors. It

[5] Joseph Peterson, Frequency and recency factors in maze learning by white rats, *J. Animal Behav.*, 7, 1917, 338-364.

was also noted in this experiment that the animals frequently showed marked conflicting tensions in critical situations in the maze.

The results of this study were confirmed by a subsequent investigation on adult human subjects involving 4518 critical reactions.[6] The results of the two investigations were so much alike that it would be difficult for any one, from a careful analysis of the several reactions, in either case to say whether he was studying human or rat 'choices.' In the case of the humans an original and improved method was used, viz., that of the mental maze which eliminates the effects of irrelevant space factors and brings the behavior into the ideational realm, but otherwise leaves the situations much alike.[7]

The mental maze method has wide possibilities in association, thinking, and learning investigations, only a few of which have yet been utilized. For example, a technique was devised which presented to adult persons a situation of such a nature that frequency and recency effects operated directly against learning, so that if these factors were in control learning would be impossible. The result was that learning went on as before, but constantly forced disruptions of the connections that had been made on the basis of frequency and recency factors. Something like Thorndike's law of effect prevailed over his law of exercise.[8] He has never given an account of how the law of effect operates neurally. His law is simply a statement of indisputable facts. Thorndike has recently published results of 26 different experiments on the effects of frequency, which seem to support from a different point of attack, our own results of 1917 and later. His present statement is brief; he promises a special monograph to appear later, which will describe the experiments and results fully.[9] "We have carried out an extensive and varied series of experiments," he says, "the results of which all seem to show that the negative is the true answer—that the mere repetition of S, no matter how many times, does not strengthen the stronger Cs at the expense of the weaker." S represents "any situation, such as a sound or word," and the Cs are for "the connections in the mind or brain whereby S evokes or is followed by $R_1$, $R_2$, $R_3$, etc.," the responses.

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[6]Peterson, Learning when frequency and recency factors are negative, *J. Exper. Psychol.*, 5, 1922, 270-300.

[7]Peterson, The backward elimination of errors in mental maze learning, *ibid.*, 3, 1920, 257-280.

[8]E. L. Thorndike, *Psychology of Learning*, 1913, 1 ff.

[9]Thorndike, A fundamental theorem in modifiability, *Proc. Nat. Acad. Sci.*, 13, 1927, 15-18.

After describing briefly certain of the experiments Thorndike says: "From these and other experiments we conclude that mere frequency of use of $S \longrightarrow R_1$ does not strengthen it at the expense of weaker connections $S \longrightarrow R_2$, $S \longrightarrow R_3$, etc. Greater relative frequency is not a selective force. To explain the facts of modifiability some influence from the consequences of a connection is required. Facilitation and inhibition among the different connections from a situation are not explainable by 'drainage' toward the stronger, and all physiological and psychological theories which involve similar forms of drainage are under suspicion." Notice that Thorndike illustrates a situation S by "the sound of a word." Therein, we believe, lies the difficulty; as we have said, he has never made clear how the influence "from the consequences of a connection" operate. His 'situation' is defined too narrowly for such an explanation.

Kuo, experimenting with rats in a maze which offered two (a long and a short) routes to the food box, found, in conformity with our own results just mentioned, that 11 of the 13 rats finally departed from the longer in favor of the shorter route, contrary to association laws. In many cases the shift from the longer to the shorter route came about suddenly. In his own words, "many rats learned to select a correct path not only *not* because of frequency or recency, or both, but in spite of them."[10] My own general observations of rats in mazes with two routes to the food agree with Kuo's. Surely the associationists (including the modern disciples who regard contiguity in conditioned reflex as the all-sufficient principle of learning) have something to explain here, since rats first forming the habit of going by the long route change so unaccountably to a different route. If this were in human psychology it could conveniently, in accordance with old and respected views, be attributed to the 'will.' The only way out of the difficulty, if we are to follow facts, would seem to be a recognition of some sort of adjustment to situations *as wholes*, rather than to stimuli *seriatim*; and this is just where association *per se* fails to serve us.

Associationism early showed its weakness in the experimental psychology of learning where it was necessary to invoke a stamping-in effect of pleasure on 'successful' acts, and a stamping-out effect of pain on 'wrong' acts. Thorndike, who employed this conception,

[10]Z. Y. Kuo, The nature of unsuccessful acts and their order of elimination in animal learning, *J. Comp. Psychol.*, 2, 1922, 17.

early recognized the difficulties involved in the fact that the pleasure would thus have to be retroactive through a chain of random activities, giving emphasis to those only which led up to the pleasurable end-result.[11] But even with such mysterious guiding powers ascribed to pleasure this hedonistic theory had to explain how elimination of errors comes about. Why was not the whole series of random acts which finally led up to the pleasurable end-reaction fixed *as a whole*, since pleasure resulted from the whole series as it occurred? Aside from these difficulties was, of course, the clear implication of an enigmatic dualism. In Max Meyer's terms, a ghost had been introduced to do what could not be understood and accounted for on the laws of nature; it was a real unconditioned force as objectionable and as unfounded in facts as any other faculty. Indeed it was only an old pre-scientific conception clothed in new terms, and it involved the fallacy, pointed out by James,[12] of confounding, in higher levels of activity, a *pleasurable act* with an *act seeking pleasure*. Learning simply is not explained by any such assumption, which only substitutes words for explanations. Whatever view we take of the conscious aspects of activity there is still lacking the evidence that consciousness as a 'thing' in any of its aspects does anything at all. The explanation of the behavior of an animal in terms of consciousness as a force is doubly removed from the demonstrable facts of science. As applied to persons, the situation is not in any important sense different. Associationism, therefore, finds little support in hedonism. This whole trouble with this old psychology, still scattered through our text-books, is that theory, not responsible to experimental verification, dominates and selects its 'facts' for the construction of 'systems' of psychology. It should be the aim of the newer science to replace these systems with tentative hypotheses so framed as to be subject to experimental verification.

There are many facts of behavior that associationism—including the extreme form of the conditioned reflex doctrine, which also goes beyond facts to a system—has overlooked. Chief among these are the tensions and strivings manifested in what has been called voluntary attention. The subjective psychologists rightly arrived at the result that there is something unitary about our responses, that we cannot do several unrelated things simultaneously; but they erred, of

[11]Thorndike, Animal intelligence, *Psychol. Monog.*, 2, 1898, no. 8, 104.
[12]James, *op. cit.*, II, 556 f.

course, in ascribing this phenomenon to attention as a unifying force. Objective psychology has likewise discovered the fact that behavior is somehow unitary; that there is a coördination of some sort among all the minor activity systems of the organism, even including reflexes. Thus Watson, who has shown enthusiasm for the form of associationism expressed in conditioned reflex literature, asserts that "even an hour's study will convince you that the human body . . . is . . . but a very commonsense kind of *organic* machine (and by organic machine we mean something many millions of times more complicated than anything man has yet succeeded in making),"[13] and that "*whenever the individual is thinking, the whole of his bodily organization is at work (implicitly)*—even though the final solution shall be a spoken, written or subvocally expressed verbal formulation."[14] The work of Cannon on emotional responses, showing large integrations of bodily changes checking or facilitating one another—some having 'right of way' over others under certain circumstances—leads to similar conclusions, as does also that of Sherrington, Dodge, and others on reflexes. Child's work on physiological dominance and the coördination of several part-activities by the dominant region in an organism, and on the regulation of pattern reactions in various equilibration processes of organismic integrations (physiological gradients) is a valuable contribution in the same line, and is encouraging toward an objective psychology deprived of all faculties.[15]

My own experiments on substitution learning, rational learning, and ball tossing[16] have revealed marked tensions under difficulties and in early stages of adjustment, and also noticeable releases and relaxations with increase in the rate of response as the adjustments became more complete and more adequate to the new situations. In many cases, but not of course in all, learning looks like forced adjustment to a situation involving simultaneously many factors; and it differs from tropism, the simplest forced adjustments, only, or at least chiefly, in the readiness of the yielding to the situation.

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[13] J. B. Watson, *Behaviorism*, 1925, 42.

[14] *Ibid*, 213.

[15] Cf. W. B. Cannon, The interrelations of emotions, *Amer. J. Psychol.*, 25, 1914, 257-282; C. S. Sherrington, *The Integrative Action of the Nervous System*, 1906; Raymond Dodge, Adequacy of reflex compensatory eye-movements including the effect of neural rivalry and competition, *J. Exper. Psychol.*, 6, 1923, 169-181; C. M. Child, *Physiological Foundations of Behavior*, 1924, especially chaps. 10-14.

[16] See especially, Limits of learning by trial and error, *J. Exper. Psychol.*, 11, 1926, 45-55.

These results are in general not far different from those brought out by Köhler on apes. The "insight" (*Einsicht*) which an animal in a difficult problem-situation, after some fumbling, rather suddenly gets and thus effects a solution, is very similar to the (more gradual) release in tension in our own human subjects on problems of complex learning. Köhler's also agree with our results in showing that the mere associative principles—as well as 'imitation'—are inadequate to explain such behavior.[17] An animal may be puzzled before the difficult situation of getting a banana placed outside the cage beyond its reach. It may make several vain attempts to reach the food and then give up direct efforts. Then rather suddenly it may pause in its wandering before a long stick, pick it up and go directly toward the banana, raking it in more or less clumsily with the stick. In such a case the situation is simple enough for the rather complex organism, practiced in different manipulation processes, so that the adjustment motivated by the inner food drive is somewhat directly effected. One of Köhler's apes (Grande) made a four-story box structure which she mounted and brought down a banana suspended far beyond her natural reach. In these cases it is of little aid to say that the animal "gets the idea." An idea is not a mysterious director of activity. "Getting the idea" is but another expression for the individual's rather direct adjustment to the complex situation: given a certain structure or organization (both innate and acquired) with the inner metabolic changes as drives and a situation not too complex for the organism, and the adjustment is unfailingly effected—either directly and quickly, or through a series of gropings up to a point where the adjustment becomes so simple and direct as to release the tensions and obstructions rather suddenly.

It is customary to say uncritically that successes in trial-and-error learning come by chance. This is of course not strictly correct. There may be chance elements in the commonly accepted meaning of that term, *i.e.* coincidences in the operation of various intra-organic and external circumstances not under any constant influence and too minute and complex for determination and prediction; but the successes are unquestionably functions of the reaction-patterns (both innate and acquired) of the organism at the time, and of the whole physical and social situation in which the reaction occurs.

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[17]W. Köhler, *The Mentality of Apes* (tr. Ella Winter), 1925, 228 ff.

Practice in reactions of various sorts reduces the inaccuracies and increases the directness of response so that in time the original trial-and-error character is largely eliminated. At this stage the individual reacts readily and in a properly coördinated fashion to any complex situation that does not contain too many new elements. It is well known, however, that even at this stage reactions of different individuals to similar situations differ materially: each individual reacts on the basis of his own unique past (genetic and behavior) history. Any careful study of the perceptual reactions of different persons will reveal rather marked individual differences verging into the larger, more or less constant errors, known as illusions. The illusions are due to variations from their usual relationships of certain elements, whether intra-organic or external. The whole class of rather well-coördinated reactions to present situations are called perceptions in the older subjective terminology; and it is well known and has been experimentally proved that 'we see things as our past experiences have taught us they must be.' Insight is but one of the higher reaches of such reactions.

In another paper[18] this behavioristic or objective view of perception has been tentatively outlined and related to learning. As early as 1907 Dodge, from his experiments on eye-movements, had come to the result that the conscious aspects of visual perception are inadequate as indicators of the reaction-patterns involved. Thus we read that "so far at least as the theory of motor local signs are [is] concerned direct consciousness of eye movements is altogether unnecessary." "Adequate sensations of eye movements simply do not exist. Whatever the motor factor in the spatial organization of the retina may be it comes to consciousness not as sensation of movement but as spatial relations."[19]

May we not say that all perceptions are really responses, since it is true that inhibited action, as well as expressed action, is 'response'? On this view the perception of any complex situation before us is an elaborately coördinated system of responses with the different relevant factors properly subordinated to some more or less long-ranged adjustment that can be completed only with the passing of a considerable time interval. Time is, of course, as much an element of reality as space; it is the *order* in which the several responses are

[18]Peterson, Local signs as orientation tendencies, *Psychol. Rev.*, 33, 1926, 218-236.

[19]Dodge, *Psychol. Monog.*, 7, 1907, no. 35, 67 ff.

taking place. It is physically impossible, for instance, to go to the shelves of a library that is visible to one, get a certain book, take it home and read it, and make its principles actualities in one's life *all in an instant, or simultaneously.* There is a certain sequence that must be followed out if these adjustments are to 'square with' the circumstances we call reality. This general truth was, of course, early recognized, and it gets rather clear expression in Berkeley's *New Theory of Vision,* (1709). In addition to this fact regarding the time element in perception is another one very closely related to it, that in all perceptual reactions only a part of the situation reacted to is physically present so as to stimulate the receptors of the subject. Moreover, very seldom are all the receptors stimulated by the relevant factors of the situation. Thus we see objects as heavy, cold, etc., without lifting or touching them. As a matter of fact in the development of various reaction-patterns, responses come gradually to be highly selective, certain elements dominating others. 'Equilibration processes' in the organism enable it to react to situations as wholes (never, however, as complete or static wholes), the facilitation and inhibition factors in the entire pattern-reaction being aroused through certain correlation centers by the stimulation of only some of the visual factors in the total situation. This is hardly a matter of mere association, though association plays a part, but of forced adjustment along lines of greatest intraorganic consistency. Different part-processes in the organism interstimulate one another while others are suppressed, so that a certain direction of activity may be maintained through long periods of time, either wholly without guidance or with very little guidance by external factors. Thus ideation in general—thinking, day dreaming, reasoning, and various abnormal forms of behavior, in particular—may be understood.

Watson has rightly stressed the importance of subvocal (language) activities in such behavior. The child's early overt reactions are inaccurate and short-ranged, directed successively and blunderingly now to this and now to that object or person. His language responses at an early age run unchecked by social consequences. Gradually they bring about conflicts resulting in inhibitions and restrictions imposed by others, and are forced to take more or less subvocal and perhaps even circular channels. They thus serve as substitutes for the more overt acts for which they come to stand. Thus in the child's play saying 'doll' in connection with appropriate movements takes

the place of actually having present and handling a real doll; and in a later stage the word need not even be spoken for subvocal activities serve as well. Thus the individual 'thinks' of remote objects and situations. He really reacts to them as being located in a certain temporal and spatial order of events to which he has learned to keep himself oriented more or less satisfactorily even though they cannot directly stimulate his receptors.

The older psychology regarded such reactions as impossible except by means of 'mental images' interposed between the individual and the external reality. Thus the doctrine of control by images arose, and volition was regarded largely as a matter of choosing and holding before one the images of the acts to be performed, to the exclusion of other images. These images with their 'meanings' constituted 'ideas,' and there was much discussion about the association of these ideas, their clearness and distinctness, etc. The existence of such images was finally challenged in part or *in toto*, until it became evident that whatever the nature of the so-called images may be, we normally react not to, or by means of, the images at all but directly to the external situations to which they are said to refer; that, in fact, images are but aspects of our reactions to real stimuli whether intra-organic or external. As in perception so in ideational behavior, or reaction to absent things or conditions, we must note that the operations of the various elements of the reaction-patterns involving subvocal activities may be wholly outside the range of the individual's awareness; that awareness of one's *complete* intraorganic adjusting processes to distant situations simply does not exist. We can, of course, be trained to react in some degree to certain of these intra-organic adjustment processes themselves (introspection), but such actions are more or less perversions of normal overt reactions to distant situations, and are not essential to them, or to our thinking—at least not to any important extent, as if the relationship were a close or highly correlated one.

Miss Washburn's theory of "persistent tentative movements" has been suggestive toward the line of thought developed in this paper, and agrees in part with the views here developed. In 1905 Watt[20] used the term *Aufgabe* for the 'idea of a problem to be solved.' This *Aufgabe* had supposedly a certain effect upon the associative

[20]H. J. Watt, Experimentelle Beiträge zu einer Theorie des Denkens, *Arch. f. d. ges. Psychol.*, 4, 1905, 289-436.

process, giving it direction. Ach,[21] in the same year designated by the same term the 'set' of the subject in the reaction experiment for a 'sensorial' or a 'muscular' reaction, according to what the instructions had been. To account for this 'set,' Ach assumed that, besides the associative dispositions and perseverative dispositions, a third factor enters in such cases. This he thought of as a 'determining disposition' or 'tendency in the nervous system' resulting from the idea of the end to be accomplished.[22] This determining tendency must, of course, be accounted for in physiological terms if one is to be clear of dualism and assumptions of mysterious influences by purely 'mental states.' Miss Washburn's theory aims to go beyond a 'solution' by the mere assumption of a spontaneous tendency of ideas to recur on the principle of perseveration, an assumption of that which was to be explained; and also beyond suppositions to the effect that emotional aspects of the problem-idea keep it in the foreground. She worked on the hypothesis that "to explain this persistence we need to invoke something over and above ordinary associative dispositions; in other words, that an associative tendency becomes a determining tendency through the operation of some factor that is not itself an ordinary associative tendency."[23] Her position is that the determining tendency or problem-idea set up is really maintained as *persisting tentative movements* which continue from the time of the stimulus to the time of the response, "or if they lapse momentarily, when there is a long wait between the giving of the instructions and the presentation of the impression [*i.e.* the response], they renew themselves spontaneously."[24] She seems to follow Sherrington in supposing that, as she herself expresses it, "the stimuli to the continued innervation of the muscles are the kinaesthetic excitations resulting from the innervation of the other muscles in the system."[25] Thus by mutual interstimulation through proprioceptive excitations, the various muscles in the system keep up either persistent tentative movements or a static movement system. The latter is illustrated by the holding of the head in a fixed erect position. In this latter case she overlooks or neglects the effects of certain constant external stimuli and also continuous reaction to the 'pull of gravity.' Her explanation does

[21] N. Ach, *Ueber die Willenstätigkeit und das Denken*, 1905.

[22] In our own way of thinking, both the idea and the tendency are aspects of the reaction, the results alike of the instructions.

[23] M. F. Washburn, *Movement and Mental Imagery*, 1916, 155.

[24] P. 156. [25] P. 158.

not get far from associationism, for she sums up with this statement: "In other words, *if the motor excitation on which an idea is based can be associated with an internal static movement system, it will acquire the persistence needed to transform the idea into a problem idea.*"[26] Also on a later page she says: "*a determining tendency is an associative disposition one of whose exciting influences is the activity attitude.*"[27] While she properly rejects Meumann's "inner assent" or "elementary active reaction of the Ego," as leaving us wholly in the dark regarding the underlying physiological processes, she never quite frees herself from similar implications of dualism. The effort frequently felt in learning is accounted for by her as association with activity attitudes, but the whole process seems somehow to be under the direction of consciousness or of an idea, either of which is possessed of some dynamic quality of its own. Fatigue, in her view, has a very prominent part in ending problem-ideas or activity-attitudes, and we are told that when such an attitude "ends too soon for the highest degree of efficiency in work, it may be because he [the worker] has the bad habit *of paying too much attention to fatigue sensations.*"[28] Consciousness seems frequently to play a rôle in her system; for she finds it necessary to account for the continuance of the influence of a problem-idea "after the idea itself has dropped out of consciousness."[29] An examination of the responses known as judgments leaves one with the same feeling of dependence upon assumptions of dualism.

She does, of course, hold that "all association is association between movements;"[30] that the essence of attention is movement; that the so-called fluctuations of attention are not changes in the object or stimulus but in our reaction to it;[31] and that for ideas to have relatively lasting influence as problem ideas they must appeal to a need or a desire. "All desires are ultimately connected with the great motor outlets of instincts."[32] But, as a matter of fact, she fails to follow out this notion of motivation by instincts, and leaves the inter-stimulating effects of tentative muscular movements set up by exteroceptive stimuli to do the main work of keeping up the mental processes. This defect, from our point of view, is evident in all the systems of psychology based upon the 'association of ideas.'

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[26]P. 159. [27]P. 162. [28]P. 167, italics ours.
[29]P. 168. [30]P. 34.
[31]P. 35. [32]P. 160.

Our own view,[33] dating back as far as 1916 in its early beginnings, is rather different: it conceives of the whole series of reactions known as mental activity—perception, ideation, judgment, reasoning, willing, etc.—as well as the more overt behavior activities as being *forced*, on the one hand, *by the 'drives' or stimulation of intraorganic (metabolic) changes*, and, on the other, *by external stimuli*; but the latter, which early develop for any individual into situations, as has been explained, present the possibilities, the outlets so to speak, of the behavior activities. They give direction to the inner metabolic drives and thus determine to a large extent the reaction-patterns that become built up in the individual through experience whether overt as choices, acts, etc., or implicit as 'thoughts,' the 'flow of ideas,' etc. It is not denied, of course, that in these activities association laws have some effect, but their effects are chiefly stereotyping in their nature. The organism, long before birth, is a 'going concern,' not some passive thing to be aroused by external stimuli. The life processes constantly use up energy, which must be replaced, and they thus destroy any equilibria that tend toward quiescence. The organism as a whole is so constituted by its inherited, 'survival' qualities that different part-activities constantly interact upon one another in such a manner as to keep all processes going in their properly balanced relationships. If stimulation and consequent movement is excessive, the waste products accumulating in the blood excite to greater activity the breathing, circulatory, excretory, temperature-reducing, and other activities, which in turn remove the intra-organic irritants, bringing the activity back to normal. If food supply fails, inner bodily changes, like contractions of the stomach-wall muscles, due to disturbed conditions of inter-cellular activity, set up sensory impulses (interoceptive and proprioceptive) which, going back through higher nerve centers and thence out to skeletal muscles, effect wandering, stalking, attacking and other food-getting movements; and these again subside as the food thus obtained becomes assimilated and the equilibria disturbed by the hunger state are reëstablished. There is no stopping of this intra-organic activity during the entire life of the individual; it is basic to all ideation, thought, volition, etc. Indeed, ideational activities are but subtler aspects of behavior and are brought about through conflicts, frustration and redirection, inhibitions, etc., of overt activities. In all this behavior, of course, the autonomic processes play a large and basic rôle as drives.

[33]Cf. *Psychol. Rev.*, 23, 1916, 153-162.

But there is no idea, ego, will, or what not, that is an original force and not simply an outcome of past circumstances. These are but aspects of the onward-moving, adjusting organismic processes, not directors of them. It is true that ideas, resolutions, will, and the others represent directive tendencies of more or less continuity, but these tendencies arise in and develop out of the life processes. The vitalists, and dualists generally, err in failing to recognize the fact that 'vital forces' are based on organization of activities that have a natural continuity. No one needs deny the 'spontaneity' of the general organismic activities, in the sense that they are initiated largely and get their 'drive' qualities from the metabolic processes of an organized life-unity that comes to any external situation from a remote past which has given it some very definite characteristics and self-perpetuating properties. Because of these inherited organizations—for food getting, sex activities, self-assertion, sleep and rest, play, etc.—each individual manifests in every situation of complex stimuli certain very definite *selective tendencies*, which become specialized or modified in various particulars by the exteroceptive stimulations.

That some of these aspects of behavior are conscious is no proof that they are arbitrary, self-directive or original forces, and certainly many of them are not conscious. Indeed, we have no way of getting at the 'ultimate meaning,' whatever that is, of this fact that some processes are conscious. We do not even know whether or not all the somewhat remotely integrative life activities have conscious aspects or even whether all organizations of 'matter' have such aspects. We cannot work profitably at the problem unless we can get some grip on it from the outside, and that we cannot get of consciousness.

It is possibly true that 'mental images' as understood by psychologists are learned reactions to certain intra-organic (largely vocal and subvocal) processes. These processes are kept up chiefly by partly-impeded muscular responses which in turn arouse proprioceptive impulses. These latter are conducted back through the cerebral channels and thence out again to responding organs. Thus the intra-organic adjustment processes may persist, as Miss Washburn has contended, for long periods of time with only slight but persistent changes as to the elementary activ ties involved; but they are constantly becoming complicated with extroceptive impulses from external stimuli. Images are probably absent in lower animals with less richly endowed sensory, mediating, and motor neural

mechanisms, and also in children who have not yet learned to react vocally as means of partially controlling other individuals, or social situations.[34] The extensive rôle which these vocalization responses, whether suppressed or overt, play in the lives of children and adults and in the development of personality is an important problem in social psychology, and has been explored somewhat by Baldwin, Cooley, and others.[35]

The loss of persistency in the overlapping intra-organic processes and the consequent alterations of personality are very noticeable in extreme fatigue. A bit of instructive experience in this regard came to the writer about a year ago, when he and four other persons were making a non-stop automobile trip of several hundred miles. The intention was to run one whole night and the following day, but tire trouble where little help was available caused a delay that resulted in the necessity of traveling most of the second night. Those who drove the car, including the writer, found it impossible while at the steering wheel to trust themselves at keeping awake while the others slept, so two men sat in the front seat simultaneously, always keeping up a conversation to keep awake. A stage of sleepiness was reached in which the integrity of personality seemed to weaken materially. One would think of something to say and immediately thereafter be unable to say whether he had said it or not, and having said what was on his mind he would also forget what the other's reaction to it was. Finally this became the object of conversation, and both observers agreed essentially in their descriptions of the experience. Emergencies, such as a small detour around a bad bridge or a broken-up roadway, would be met by the driver in a peculiarly reflex manner. He would feel as though he himself was not steering the car and shifting the gears. These activities would come much as does a sneeze, forced by an immediate situation, and all awareness of them would be so immediately forgotten that the several acts would not seem to

[34]See W. S. Hunter, The problem of consciousness, *Psychol. Rev.*, 31, 1924, 1-31; K. S. Lashley, The behavioristic interpretation of consciousness, *ibid.*, 30, 1923, 237-272, 329-353; C. Rosenow, Behavior and conscious behavior, *ibid.*, 30, 1923, 192-216; A. P. Weiss, Behaviorism and behavior, *ibid.*, 31, 1924, 32-50; 118-149.

[35]Cf. J. M. Baldwin, *Mental Development: Methods and Processes*, 1895, chaps. The Mechanism of Revival, Internal Speech and Song, 3rd (revised) edition 409-419; *Social and Ethical Interpretations*, 1897, especially chap. 4; C. H. Cooley, *Human Nature and The Social Order*, 1910, especially 157 ff.; F. H. Allport, *Social Psychology*, 1924, chap. 8; W. James, *Varieties of Religious Experience*, 1902, chaps. The Sick Soul, The Divided Self, etc. The *Book of Job* is interesting from this view-point, as are all 'moral' struggles.

be connected as one's own behavior. The experience was different from the automatic gear-shifting that may be done in normal consciousness while one is thinking of remoter matters: it was more like the protective reflex adjustments which take place when one slips on the sidewalk, with the addition that the self-thought and concern and all remote considerations are largely absent. There is a good deal of similarity between this state and some of the disorganizations of the hysteric. The normal individual, on the contrary, is always, in waking hours, responding to a group of remote circumstances more or less related to one another, which give him not only a 'specious present' but also a coördination with respect to remote circumstances. This is the result of an overlapping of responses (largely subvocal) as already discussed. In extreme fatigue resulting from the loss of sleep there does not seem to be enough vigor for such comprehensive responses which always involve various strains of inhibition and conflicts, and so each present stimulus acts on the organism more nearly in a seriatim fashion.

All resolving or deciding responses, like choice, judgment, reasoning, volition, etc., are forced reactions brought about by obstructions of such a nature that the organism cannot continue smoothly its line of activity. They result from the fact that circumstances offer several ways out of a predicament, while the organism, being a single body, cannot follow out all of them. It can take only one direction of overt activity. Several anticipatory adjusting processes may conceivably, and actually do, tend simultaneously in different directions (consider 'struggles with temptations,' doubtings, negativistic tendencies, schizophrenias, suppressed and otherwise more or less dissociated personalities), and thus bring about among one another actual physical tensions and conflicts in muscular systems. These are experienced by the individual as resolutions, judgments, decisions, etc., and about their reality there can be no doubt whatever; the organism is a unitary organization of inter-acting and equilibrating processes which actually does bring about in different situations, into which it is forced, *new results*. Every individual is a unique organization in the universe, which reorganizes and gives direction and unity to diverse processes affecting it. As individuals we do actually decide, resolve, and bring things to pass by these inner struggles and strivings which the philosophers have noted and, in our view, often misinterpreted. Our volitions and choices are such deciding responses; and since each individual is different from every

other one his decisions are real and unique, bringing about new events in the universe. They are, however, not unconditioned powers or arbitrary forces, but the natural results of an inherited and acquired constitution and of the objective situations into which the individual has been forced. One's several decisions and conclusions may be anticipated more or less accurately by any careful student of their genesis; rationality means a tendency to consistency with objective circumstances. The universe and our organisms are so complex, however, as always to leave room for 'surprises,' unexpected outcomes of our strivings, so that it does not matter whether or not a hypothetical 'all-knowing' mind could find our behavior completely determined and mechanistically conditioned and could predict it to the finest details. There is no such mind known to us. We are robots, as McDougall in his horror for mechanism interprets it, only in his imagination. Science at its best can only roughly determine the outcome of our activities under given conditions: the finer aspects of the activities will doubtless ever be unpredictable and uncontrollable because of the complexity of variables, both intra-organic and extra-organic. The old 'absolute freedom' is doubtless a myth, but freedom as a reality can only mean relative independence of control by immediate and temporary circumstances. The more complete and adequate the innate organization and training for long-ranged, consistent behavior, the further one is from compulsion by immediate 'necessities' and the freer he is. A large degree of freedom in this sense is certainly possible to persons with good ability and opportunities for training; and such training is of supreme importance to the individual, not, of course, in a mere commercial sense but in the sense of a 'fullness of life,' of the intellectual freedom that is the *summum bonum*. But alas, the reaction systems for such behavior cannot forever maintain themselves; they give way with the wear of years. Life is not a static but an adjusting process; and the struggle for freedom is never-ending, but it is passed on from generation to generation. Our adjustment mechanisms are obviously evolutions of coördinating systems *in nature*, not impositions *upon* nature. How this process of organismic evolution has gone on and how it can be further facilitated are important problems. Arbitrary mental facilities, as contrasted with adjusting organismic mechanisms, are simply unthinkable at present; but associationism, the first really effective antidote to faculty psychology, must give up its claims as a complete interpreter of our acting and thinking.

# THE EFFECTS OF CONTINUOUS MENTAL WORK

By A. T. Poffenberger, Columbia University

The material here reported is a portion of an extensive study of 11 forms of work—7 physical and 4 mental—carried out on eleven graduate students in psychology over a period of many months. The forms of mental work were selected in part as a result of the observation of Dodge[1] that ordinary mental work used in fatigue tests really involves a great variety of functions and does not constitute the continuous exercise of a single function as in the case of the finger ergograph and other forms of physical work. Another factor determining the choice of material was the availability of a sufficient amount of it, standardized and equivalent in difficulty, to keep subjects at work for sufficiently long periods of time. Still another factor was the relative freedom from practice effect, or the feasibility of allowing for it. Mental multiplication, frequently used in such studies, is especially subject to improvement from practice and from the discovery of short-cuts, etc. With these facts in mind the following tasks were chosen: (1) continuous addition, (2) judgment of compositions, (3) completion of sentences, and (4) intelligence tests.

(*1*) *Continuous addition.* As a simple form of mental function that could be carried on for a long time, the type of addition described by Starch[2] was chosen.

This consists of adding to a two-place number, announced by the experimenter, first 6, then 7, then 8, then 9, then 6, and so on for a period of 30 sec. At the beginning of each 30-sec. period a new two-place number is announced and the operation repeated. When the sum reaches 100 or more *S* drops the third figure. All two-place numbers ending with zero are omitted. Sheets were prepared containing these key-numbers together with the series arranged in columns. *E* noted on this sheet the errors and the point reached at the end of each 30 sec. Preliminary trials showed that this material was so easy for the *S*s and their speed of adding so great that *E* could not keep an accurate record of errors. For this reason the task was changed to the addition of 16, 17, 18, 19, 16, etc. The other parts of the procedure were the same as described above. When *S* had run through the whole series of available numbers he was started at the beginning again. Material was thus available for 5 to 6 hours of work, but no *S* was able to continue

[1] R. Dodge, The laws of relative fatigue, *Psychol. Rev.*, 24, 1917, 89 ff.
[2] D. Starch, *Experiments in Educational Psychology*, 1919, 193 ff.

so long. In order to eliminate the effects of practice as far as possible from the regular work-curve, 2 half-hour practice periods were given on days preceding the experiment.

After *S* had refused to work any longer he was given a rest period of 10 min. This was again followed by a section of work sufficiently long to get a measure of capacity after rest; namely, ten 30-sec. periods. As it was impossible to keep all *S*s working a full 5½ hours, as in the other forms of work, the duration of the experiment differs for the various *S*s. Table I shows in the first column the numerals designating the *S*s, in the second the number of 30-sec. periods up to the rest period, and in the third the total number of additions. It will be noted that one *S* (XII) worked about four times as long as another (II), and that some *S*s did twice as much work as others per 30-sec. period.

TABLE I

INDIVIDUAL DIFFERENCES IN CONTINUOUS ADDITION

| *S* | No. 30-sec. Periods up to Rest Period | Av. No. Additions per 30-sec. Period | Total Additions |
|---|---|---|---|
| I | 205 | 11.8 | 2419 |
| II | 142 | 5.3 | 753 |
| III | 358 | 5.8 | 2076 |
| IV | 260 | 8.6 | 2236 |
| VI | 538 | 9.3 | 5003 |
| VII | 251 | 12.4 | 3112 |
| VIII | 287 | 8.7 | 2497 |
| X | 419 | 8.1 | 3394 |
| XI | 566 | 7.1 | 4019 |
| XII | 570 | 6.1 | 3477 |

The records for 10 *S*s are presented in Table II. Each *S*'s work up to the rest period is arbitrarily divided into 14 equal parts indicated in the Table as 'sections.' The figures under each section represent the average number of correct responses per 30-sec. period. Since the work-curves differ in length for each *S* the number of 30-sec. periods within the sections varies also. Each section equals one-fourteenth of each *S*'s work-curve, regardless of the length of the curve.

Table II will furnish an idea of the different levels upon which the *S*s worked and the changes for individuals in the course of the whole experiment. Averages are given at the bottom of the table. No reliabilities have been computed for them; but it is obvious that they would be relatively unreliable because of the different levels of efficiency upon which the *S*s worked, as indicated in this table and in Table I.

## TABLE II

### WORK-CURVE FOR CONTINUOUS ADDITION

Average number *correct* per 30-sec. period. The total number of 30-sec. periods up to Rest is divided by fourteen for each *S*. Hence the figures in this table do not represent the same amount of work for each *S*

| *S* | Section | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 |
| I | 13.47 | 13.67 | 11.67 | 12.60 | 13.34 | 14.13 | 10.87 | 13.47 | 13.60 | 12.13 | 9.50 | 8.14 | 9.21 | 9.50 | 12.42 |
| II | 6.09 | 6.18 | 4.27 | 5.45 | 5.55 | 7.00 | 5.64 | 5.73 | 5.73 | 4.55 | 6.00 | 4.00 | 3.70 | 3.60 | 5.10 |
| III | 6.50 | 5.73 | 6.46 | 5.46 | 5.81 | 5.46 | 5.19 | 5.38 | 5.52 | 5.24 | 5.96 | 5.72 | 6.47 | 5.92 | 6.30 |
| IV | 8.53 | 9.53 | 9.21 | 8.68 | 10.10 | 8.26 | 9.84 | 8.42 | 8.33 | 9.28 | 6.78 | 7.67 | 8.78 | 7.33 | 9.90 |
| VI | 9.18 | 10.49 | 10.79 | 10.87 | 10.05 | 10.44 | 9.34 | 9.58 | 8.42 | 9.08 | 8.63 | 8.29 | 7.29 | 7.16 | Rest —— |
| VII | 13.40 | 12.56 | 13.50 | 14.11 | 12.39 | 12.56 | 11.67 | 13.89 | 11.89 | 12.28 | 11.89 | 12.83 | 12.00 | 8.76 | 10.60 |
| VIII | 8.81 | 8.90 | 9.71 | 9.24 | 9.48 | 9.81 | 9.33 | 8.45 | 8.45 | 8.00 | 7.90 | 8.40 | 7.15 | 6.85 | 5.00 |
| X | 9.60 | 8.05 | 8.05 | 8.01 | 8.53 | 9.13 | 8.67 | 8.37 | 7.50 | 7.43 | 8.17 | 7.40 | 7.63 | 6.31 | 6.30 |
| XI | 7.17 | 7.49 | 7.41 | 6.95 | 7.12 | 6.98 | 7.10 | 7.05 | 6.88 | 6.88 | 7.38 | 7.08 | 7.28 | 7.05 | 6.80 |
| XII | 5.63 | 6.10 | 6.29 | 5.90 | 6.20 | 6.39 | 6.41 | 6.15 | 6.49 | 5.98 | 5.75 | 5.88 | 6.23 | 5.93 | 6.20 |
| Av. | 8.84 | 8.87 | 8.74 | 8.73 | 8.86 | 9.02 | 8.41 | 8.65 | 8.28 | 8.09 | 7.80 | 7.54 | 7.47 | 6.84 | 7.62 |

In order to correct for this difference in level and to show the general trend of change in the course of work, the data for each *S* have been transformed into percentages of the first section of his curve. The averages of these percentages for continuous addition are shown in the first column of Table VII and also in the curve of Fig. 1, marked 'Task 1.' The curve maintains a uniform level up to Section 6, when it gradually falls to 80% of the original score just before the rest period is introduced. It should be recalled that every *S* worked as long as he possibly could, and that when this point was reached the actual output had fallen only 20%. The introspective reports[3] which each *S* made of his condition at intervals during the work show consequently a wide divergence from his condition as indicated by output. This is in line with the results of other investigators.[4]

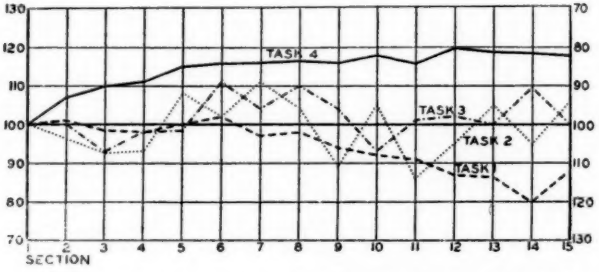


FIG. 1. WORK-CURVES. Expressed in terms of percent of score in Section 1. Task 2, which is in terms of errors, is plotted on the right-hand scale.

It might be argued that the 20% reduction represents a genuine loss of efficiency which the *S*s can recover from in part but not wholly in 10 min. of rest. For the figures show that the curve rises only 8% after rest. Continuous addition represents the closest approach to the exercise of a single function in our series of tasks and hence should be expected to show the greatest change.

Practice effect can not, of course, be entirely ruled out, although there is little or no evidence of practice effect in the early sections of the work, before fatigue should begin to show itself.

(*2*) *Judgment of Compositions.* In the second part of the experiment the task consisted in judging the quality of 130 specimens of composition[5] upon the Hillegas Composition Scale.

All the specimens had been previously evaluated by a group of competent judges. There were 100 specimens dealing with different topics, divided into 10 groups equal in size and difficulty of judgment. There were also 30 compositions on the same topic, divided into groups of 10, but not equated for difficulty.

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[3]To be published in a later paper.

[4]See E. L. Thorndike, The curve of work and the curve of satisfyingness, *J. App. Psychol.*, 1, 1917, 265 ff.

[5]For this material and that of Tasks 3 and 4 the author is indebted to Professor E. L. Thorndike.

One or more days before the experiment each *S* was given a practice period in which he was permitted to study the Hillegas Scale and to judge the set of 30 compositions mentioned above.

The experiment consisted in first judging the three sets of 10, and then judging the 10 sets of 10 over and over as often as necessary. As there were 100 specimens memory seemed to play no part, except in a few isolated cases where the *S* said he remembered a score previously given. At the end of approximately 4 hours and 45 minutes a rest period of 10 min. was introduced, followed by the judgment of one more set of 10, and the preliminary group of 30. There was no effort to make the rate of judging uniform for the various judges, so that they did widely varying total amounts of work.

The figures in Table III show both the total number of judgments made and the average time per set of 10.

TABLE III

INDIVIDUAL DIFFERENCES IN THE JUDGMENT OF COMPOSITIONS

| *S* | Total Number Judgments | Average Time per set of 10 (in min.) |
|---|---|---|
| I | 260 | 7.3 |
| II | 1430 | 2.1 |
| III | 370 | 8.1 |
| IV | 350 | 8.6 |
| VI | 260 | 11.5 |
| VII | 410 | 7.3 |
| VIII | 600 | 5.0 |
| IX | 450 | 6.7 |
| X | 830 | 3.6 |
| XI | 330 | 9.1 |
| XII | 370 | 8.1 |
| XIII | 360 | 8.3 |

In order to make the records comparable for the various judges and to allow comparison with the other kinds of tasks, the total number of sets of 10 compositions for each *S* up to the rest period was divided into 14 sections. Thus the number of sets of 10 in a section varied from *S* to *S*. In the cases where the total number of sets could not be evenly divided by 14, the extra cases were arbitrarily put into the earlier sections. For example, in the cases where there were only 26 sets of 10, the first twelve sections contained 2 sets each, and the other two contained one set each. This method of grouping does not seem to distort the facts seriously, if at all.

The results are expressed here in terms of the average error of judgment per set of 10 specimens; although time records were also kept. There was a great increase in speed of judgment as the experiment progressed in the case of all but one *S*, the average time per set of 10 at the beginning being 11.25 min. and at the end 4.03 min.

It was understood that enough time was to be taken to examine the specimens and to judge them carefully. Increase in speed due to relaxation of care and effort should show itself in increased errors of judgment, while increase in speed without such increase in errors would mean increased efficiency of judgment.

No allowance is made for practice effect in the error records. In the preliminary practice in which three sets of material were judged there was not only no improvement but a slight increase in average errors from the first to the third set, namely, from 9.2 to 10.4 units. In an experiment using similar material Thorndike, McCall and Chapman[6] also found no evidence of practice effect in terms of error records; although they noted a change in speed of judgment similar to those here reported.

The error records for the experiment are shown in Table IV, in which the scores for the preliminary and final 3 sets of 10 are omitted for lack of space. Here, as in Table II, averages are given for the different sections; but they are even less reliable than those of Table II because of the difference in level of performance of the different subjects. The table is given in order that individual records may be followed.

The data in Table IV have been expressed in terms of the percent of score in the first section, as in the case of Continuous Addition. These figures will be found in the second column of Table VII, and also in the curve marked 'Task 2' in Fig. 1. It should be remembered, in examining these figures, that increase of errors means a poorer record. Hence, in order to avoid confusion in Fig. 1, the percentages for Composition have been plotted on the reverse percent scale which appears at the right of the chart. Thus a drop in any curve means reduced efficiency while a rise in the curve means increased efficiency.

There is little evidence of consistent change in the course of this curve. The last section before the rest period is 4% poorer than the first section. There is a change from rest amounting to 8%, which carries the curve 4% above the original level. This change might be interpreted as an indication of the practice that would have appeared if it had not been hidden by fatigue. Such a conclusion is scarcely warranted, however, on account of the irregular course of the curve taken as a whole.

The material in this task involves the exercise of the judgment function upon material that is uniform in general character although

[6]E. L. Thorndike, W. A. McCall, and J. C. Chapman, Ventilation in relation to mental work, *Teachers Coll., Columbia Univ. Publ.*, no. 78.

## TABLE IV

### WORK-CURVE FOR JUDGING COMPOSITIONS

Average error per 10 judgments

| S | Section | | | | | | | | | | | | | | Rest |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 |
| I | 13.2 | 12.6 | 12.6 | 9.7 | 12.8 | 12.8 | 11.0 | 8.2 | 15.4 | 9.3 | 14.6 | 10.0 | 6.7 | 13.1 | — |
| II | 9.5 | 10.9 | 10.2 | 10.2 | 9.7 | 10.5 | 9.6 | 10.4 | 10.5 | 10.1 | 19.3 | 14.8 | 10.6 | 10.0 | 8.9 |
| III | 7.1 | 7.0 | 6.7 | 8.5 | 9.2 | 9.0 | 6.9 | 7.4 | 9.8 | 8.2 | 8.9 | 9.9 | 7.8 | 7.0 | 8.2 |
| IV | 9.4 | 7.6 | 14.8 | 10.8 | 10.5 | 6.7 | 10.2 | 11.0 | 12.3 | 9.2 | 9.1 | 9.5 | 6.9 | 9.3 | 8.8 |
| VI | 8.6 | 8.7 | 7.2 | 10.4 | 6.3 | 9.2 | 4.3 | 9.3 | 9.4 | 11.8 | 8.1 | 10.0 | 11.4 | 5.8 | 7.8 |
| VII | 10.3 | 8.7 | 8.8 | 7.7 | 8.8 | 9.7 | 8.0 | 8.2 | 9.6 | 11.3 | 12.5 | 7.7 | 9.8 | 10.3 | 9.2 |
| VIII | 10.1 | 11.3 | 8.8 | 12.4 | 9.1 | 10.4 | 10.7 | 11.1 | 12.1 | 9.9 | 9.3 | 12.2 | 11.0 | 10.8 | 12.0 |
| IX | 7.4 | 9.6 | 6.3 | 7.9 | 6.5 | 7.4 | 7.3 | 7.6 | 7.1 | 6.5 | 10.0 | 7.5 | 7.5 | 7.1 | 5.1 |
| X | 20.5 | 21.5 | 19.3 | 14.5 | 14.4 | 13.4 | 12.0 | 13.1 | 12.7 | 12.5 | 13.0 | 11.6 | 11.8 | 11.9 | 17.9 |
| XI | 10.9 | 11.7 | 21.6 | 15.7 | 8.6 | 8.5 | 7.0 | 12.2 | 10.7 | 7.9 | 8.8 | 6.1 | 12.3 | 14.6 | 18.0 |
| XII | 12.8 | 11.3 | 13.3 | 14.1 | 11.2 | 11.7 | 11.7 | 11.6 | 16.4 | 11.6 | 11.7 | 14.1 | 14.1 | 18.0 | 1.7 |
| XIII | 8.1 | 10.0 | 7.9 | 9.2 | 7.6 | 10.8 | 10.9 | 7.6 | 10.3 | 7.4 | 10.9 | 12.6 | 5.9 | 11.7 | 9.5 |
| Av. | 10.7 | 10.9 | 11.5 | 10.9 | 9.6 | 10.0 | 9.1 | 9.8 | 11.4 | 9.6 | 11.4 | 10.5 | 9.7 | 10.8 | 9.8 |

differing in subject matter. Whether such work could be interpreted as the exercise of a single function is certainly open to doubt. At any rate, it may be concluded that the process of judging as here carried on causes little or no decrease in the quality of the work in the course of 5 hours. It must be remembered, however, that along with this uniform quality of production there was a considerable increase in speed with which the work was done. If this speed factor be taken into account, as it should be in a consideration of efficiency, then the negative result becomes one of great improvement of efficiency in the course of 5 hours of work. Just how much improvement has occurred cannot be readily stated in figures as there is no generally accepted way of combining speed and accuracy records.

(*3*) *Completion of sentences.* Thirty-nine sets of completion material were taken from the Thorndike Entrance Examinations for High School Graduates, Parts II and III.

These were arranged into 15 groups or sections, each (except the first one which contained only 12) containing from 24 to 36 sentences, so as to make the sections at least roughly equivalent in difficulty and in time required. Twenty-one minutes were allowed for each section, except the first, and the measure of efficiency was the amount correctly completed in that time. These times were made considerably shorter than allowed by Thorndike, in order that no one should be able to finish a section before time was called.

At the end of Section 14 a rest period of 10 min. was introduced, which was followed by the 15th section. Thus the total time required for the experiment exclusive of the rest period was about 5¼ hours.

Because it was not possible to make the various sections of the material exactly equivalent in difficulty, these sections were rotated so that no two *S*s were working on the same material at the same time. One exception occurred in the case of the first of the 14 sections of the work which contained only 1 set of 12 sentences and was the same for all *S*s. This constituted a kind of practice trial.

The records for this part of the experiment are presented in Table V, where all figures are given in terms of percent of the maximum-possible score, after a correction has been made for differences in difficulty[7] of the several sections.

It was thought advisable to express all figures in terms of the percentage of maximal score because the actual score for the various sections varied according to the number of sentences they contained. Since Section 1 contained the same material for all subjects these scores will give an indication of individual differ-

[7]This correction was made as follows. Each *S*'s records were first expressed in terms of the percent of his average score. These percentages were then collected for each group of material, regardless of subject or position in the work-curve, and averaged. The correction necessary to equalize these averages was then obtained and applied to all scores.

## TABLE V

### WORK-CURVE FOR COMPLETION OF SENTENCES

Percent of maximum score possible, corrected for differences in difficulty

| S | Section | | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 |
| I | 10.3 | 25.3 | 18.7 | 36.6 | 18.1 | 29.6 | 31.4 | 30.5 | 21.5 | 25.4 | 37.5 | 29.8 | 10.3 | 29.3 | 20.9 |
| II | 72.3 | 54.6 | 54.2 | 58.2 | 55.8 | 53.0 | 68.1 | 44.7 | 47.9 | 41.3 | 51.1 | 53.9 | 61.3 | 52.3 | 44.1 |
| III | 34.5 | 43.3 | 49.5 | 37.5 | 48.1 | 51.4 | 42.2 | 55.0 | 54.2 | 50.0 | 49.4 | 46.7 | 36.6 | 51.3 | 44.0 |
| IV | 62.0 | 35.6 | 41.4 | 37.9 | 45.5 | 70.0 | 41.7 | 38.1 | 49.1 | 39.2 | 45.8 | 59.4 | 48.8 | 47.8 | 62.8 |
| VI | 48.2 | 48.3 | 35.6 | 29.7 | 30.9 | 35.6 | 40.1 | 51.0 | 30.2 | 41.5 | 42.5 | 27.7 | 34.8 | 34.3 | 30.6 |
| VII | 68.9 | 81.9 | 71.6 | 67.7 | 51.9 | 64.5 | 60.1 | 44.7 | 71.3 | 64.4 | 51.6 | 52.3 | 63.6 | 63.5 | 61.7 |
| VIII | 62.0 | 50.4 | 37.3 | 62.0 | 54.6 | 57.4 | 49.1 | 54.6 | 55.8 | 44.8 | 61.7 | 57.9 | 59.7 | 60.3 | 49.9 |
| IX | 68.9 | 65.3 | 64.9 | 62.2 | 69.8 | 66.0 | 64.2 | 70.7 | 70.5 | 51.2 | 57.0 | 55.0 | 80.1 | 61.7 | 63.8 |
| X | 10.3 | 49.1 | 36.3 | 36.1 | 40.5 | 41.7 | 34.9 | 51.1 | 38.9 | 39.9 | 35.3 | 50.6 | 40.7 | 38.3 | 40.9 |
| XI | 44.8 | 51.9 | 47.8 | 49.0 | 52.8 | 53.5 | 62.7 | 57.8 | 57.8 | 48.3 | 40.1 | 55.8 | 56.0 | 59.7 | 52.6 |
| XII | 41.3 | 30.9 | 25.3 | 28.0 | 43.1 | 36.8 | 44.8 | 36.6 | 38.1 | 22.5 | 37.3 | 25.0 | 43.1 | 38.8 | 37.5 |
| XIII | 54.6 | 56.0 | 76.4 | 59.4 | 61.2 | 73.7 | 51.1 | 95.4 | 76.0 | 75.8 | 55.6 | 62.8 | 60.8 | 80.1 | 69.2 |
| Av. | 48.2 | 49.4 | 46.6 | 47.3 | 47.7 | 52.9 | 49.2 | 52.5 | 50.9 | 45.4 | 47.8 | 48.8 | 49.7 | 52.3 | 48.2 |

Rest

ences in ability to complete sentences without special practice. In two cases, Subjects I and X, there was a misunderstanding of the instructions with the result that each made only 10.3% of the maximal score possible in that section. On account of this fact other computations in this report will be based on Section 2 instead of Section 1.

Although averages for sections are presented in Table V no reliabilities have been computed for them, and our interpretation will be made on the average percent of change, as in the two preceding tasks. As stated above, these percentages are computed on the second section instead of the first, because of the irregularity of performance in the first section. These data are given in column 3 of Table VII, and are presented graphically in Fig. 1, Task 3.

No consistent change is apparent in the course of the curve, half of the points falling above, and half on or below, the line indicating the original record in an irregular fashion. Little significance is to be attached to the fact that the final section before 'rest' is 10% above the base and that after 'rest' the curve returns exactly to the base line, because of the variation throughout the curve. It must be concluded from these figures that there is no well-established loss of output from approximately 5 hours of work at completion of sentences. Neither this task nor the next preceding was continued to exhaustion or until the *S* refused to work longer. On the other hand, it was stopped only when no more material was available. What the result might have been if it could have been carried on longer, as in the case of Continuous Addition, the writer cannot say.

(4) *Intelligence tests.* The fourth task consisted of 14 forms of Part I, Thorndike Intelligence Examination for High School Graduates.

These forms are almost exactly equivalent in difficulty and can be made exactly so by the use of a set of corrections. Each form contains 13 tests composed of from 5 to 20 separate tasks. The completion of one of the 14 forms constitutes a section of the work-curve. All *S*s began the experiment with the same form, but thereafter the forms were rotated in such a way that no two *S*s were working on the same form at the same time.

The time allowance for each form was reduced from the 30 min. specified by Thorndike to 21 min. in order that no *S* could finish before time should be called and thus be able to rest. The time to be allotted to each of the 13 tests of a form was determined empirically.

Some duplications of material were discovered in certain tests of the various forms, especially in the case of problems making up the arithmetical series. As far as could be discovered from a careful examination of the scores for forms containing such duplication, no account need be taken of them. No test where there was duplication had a time-limit of more than 2 min.; so that the gain from repeating one or two examples in the series would not affect the performance in the form as a whole. After approximately 5 hours a rest period of 10 min. was introduced, which was then followed by the 14th section of the work.

## TABLE VI

### WORK CURVE FOR THORNDIKE INTELLIGENCE EXAMINATION

Records are expressed in terms of percent of maximum score (153) possible in each trial

| *S* | Section | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 |
| I | 58.8 | 58.8 | 59.5 | 61.0 | 60.1 | 59.5 | 67.0 | 64.0 | 65.4 | 71.9 | 57.8 | 66.0 | 73.9 | 68.6 |
| II | 63.4 | 69.9 | 73.2 | 74.5 | 79.4 | 72.9 | 73.9 | 77.8 | 80.1 | 74.5 | 69.3 | 82.4 | 83.7 | 77.1 |
| III | 67.3 | 68.7 | 72.2 | 74.5 | 71.2 | 79.4 | 76.1 | 67.3 | 58.2 | 67.0 | 69.3 | 73.9 | 71.2 | 77.8 |
| IV | 77.8 | 79.1 | 83.7 | 80.7 | 85.3 | 88.9 | 81.0 | 84.0 | 79.7 | 84.3 | 83.7 | 83.7 | 69.9 | 83.7 |
| VI | 60.1 | 60.8 | 66.7 | 63.1 | 70.6 | 71.9 | 68.6 | 73.9 | 68.6 | 71.9 | 76.5 | 74.5 | 72.2 | 74.8 |
| VII | 86.2 | 89.2 | 88.6 | 90.0 | 89.5 | 82.7 | 88.2 | 90.8 | 88.9 | 99.3 | 91.5 | 96.1 | 96.1 | 93.5 |
| VIII | 62.7 | 80.1 | 72.5 | 70.6 | 71.6 | 78.4 | 79.1 | 83.0 | 88.9 | 77.8 | 78.4 | 80.4 | 81.7 | 82.0 |
| X | 83.0 | 79.7 | 86.6 | 78.1 | 82.4 | 77.8 | 80.7 | 76.5 | 90.2 | 85.6 | 91.5 | 89.5 | 82.4 | 86.9 |
| XI | 58.2 | 54.9 | 64.7 | 69.9 | 69.9 | 67.3 | 62.1 | 72.5 | 61.4 | 69.6 | 66.3 | 69.9 | 71.2 | 65.0 |
| XII | 49.0 | 68.6 | 66.7 | 77.8 | 83.7 | 85.6 | 85.0 | 81.0 | 84.0 | 82.0 | 79.7 | 79.7 | 84.6 | 79.1 |
| XIII | 83.7 | 84.0 | 81.7 | 79.7 | 87.6 | 89.6 | 94.1 | 91.5 | 87.6 | 88.2 | 96.4 | 88.6 | 88.9 | 82.4 |
| Av. | 68.5 | 72.1 | 74.4 | 75.1 | 77.6 | 78.0 | 78.4 | 78.9 | 77.9 | 79.3 | 78.5 | 80.3 | 80.4 | 79.7 |

Rest

The records for this task are presented in Table VI, in terms of the percentage of the maximal score possible, namely, 153 points. This method of expressing the results was chosen in order that they might be similar in form to those of Task 3. Averages are shown at the bottom of the table.

As in the three preceding tasks, these data are also expressed in terms of the percentage of the score in Section 1 and are shown in Column 4 of Table VII, as well as in the curve marked 'Task 4' in

TABLE VII

THE EFFECTS OF CONTINUOUS WORK

Records are in terms of percent of score in first sections

| Section | Continuous Addition | Composition | Completion | Intelligence Tests |
|---|---|---|---|---|
| 1 | 1.00 | 1.00 | —— | 1.00 |
| 2 | 1.01 | 1.03 | 1.00 | 1.07 |
| 3 | .99 | 1.07 | 0.93 | 1.10 |
| 4 | .98 | 1.07 | 0.98 | 1.11 |
| 5 | 1.00 | 0.92 | 0.99 | 1.15 |
| 6 | 1.02 | 0.98 | 1.11 | 1.16 |
| 7 | 0.97 | 0.89 | 1.04 | 1.16 |
| 8 | 0.98 | 0.96 | 1.10 | 1.17 |
| 9 | 0.94 | 1.11 | 1.04 | 1.16 |
| 10 | 0.92 | 0.95 | 0.93 | 1.18 |
| 11 | 0.91 | 1.14 | 1.01 | 1.16 |
| 12 | 0.87 | 1.05 | 1.02 | 1.20 |
| 13 | 0.87 | 0.95 | 1.00 | 1.19 |
| 14 Rest | 0.80 | 1.04 | 1.10 | —— |
| 15 | 0.88 | 0.94 | 1.00 | 1.18 |

Fig. 1. The curve takes a gradually rising course, not unlike that of a learning-curve, so that at the end of the work period just before 'rest' it stood almost 20% above the starting point. The score was practically the same after 'rest' as before.

Obviously some account must be taken of practice in this part of the experiment, and the extent of it to be expected under ordinary circumstances may be learned from a study of this same material by Race,[8] who tested a group of 43 *Ss* of superior intelligence with 15 forms taken one per day for 15 days. Her curves show improvement as follows: 90, 105.8, 112.1, 113, 106.5, 113.7, 108.8, 112.4, 115.3, 118.5, 108.4, 111.6, 109.9, 113.8, 110.

[8]H. V. Race, Improvability: its intercorrelations and its relations to initial ability, *Teachers Coll., Columbia Univ. Publ.* no. 124.

There was a net change of 20 points in score or about 22%. Race's practice-curve is very similar in general form to our work-curve; although improvement of her *S*s is somewhat more rapid in the first few trials. It seems safe to conclude in this case that there is no loss of efficiency in terms of output from 5 hours of work of this type, since the work-curve resembles a practice-curve so closely, and especially since there is no change whatever resulting from the introduction of 10 min. rest. If there had been cumulative fatigue the curve should form a kind of compromise between a practice-curve, such as we might expect under the circumstances, and a fatigue-curve. As in the three tasks already considered there is a striking discrepancy between objective records and subjective report. In all cases the *S*s, toward the end of the work, showed dissatisfaction, irritation, and a strong desire to give up. There was sufficient motivation through the remuneration to which each *S* was entitled for each hour of work, and through social stimulation to prevent any actual desertions.

These 14 forms of the Intelligence Examination offered varied activity in contrast to the other three tasks and especially the first, Continuous Addition. In each form there were 13 sections, each composed of a number of problems varying somewhat among themselves. In fact, the work represents about as much variation as one can readily introduce into a paper and pencil test. We are, then, in this case about as far as it was feasible to get from the exercise of a single function. There is no objective evidence whatever of fatigue from prolonged work.

## Summary

The points brought out in this report may be briefly summarized as follows.

(1) As in other studies of a similar nature there are discrepancies between objective and subjective report of the effects of continuous work.

(2) The amount of reduction in output was relatively slight even in the most extreme case, namely, Continuous Addition, where it was about 20%, and where less than half of it was recovered from in a 10-min. rest. In the Judgment of Compositions there was no clear decrease in quality of work; although there was an increase in quantity of work done of a given quality. In Completion of Sentences there was no definite change noted; but the results are possibly

complicated somewhat by improvement from practice. In the Intelligence Tests there was no reduction in output resulting from the long-continued work.

(3) It is suggested by the results of this experiment that the more nearly the work involves the continuous exercise of a single function the more will output of work decrease, and the more varied the functions involved in the work the less will output of work decrease. Continuous Addition, which is simple and uniform in character, caused a loss of about 20% in output, and the Intelligence Tests, which are highly varied in character, showed an increase of output of approximately 20%.

(4) Further inquiry should be made as to the real cause of these differences. The question might be raised whether the difference is really due to the difference in variety of function or to the difference in the degree to which the task offers chances for a 'let down' or rest at frequent intervals. In Continuous Addition such chances were reduced to the minimum, while in doing an Intelligence Test one has to turn pages, pass from one problem to another, and from one kind of work to another. Compared with the time required to do one problem these short 'rest intervals' might make no measurable difference in score but might be all that are required to prevent the onset of fatigue. The present study suggests the latter interpretation but, of course, can offer no conclusive evidence concerning it.

# THE 'SIMILARITY' FACTOR IN RETROACTION[1]

By EDWARD S. ROBINSON, Yale University

## THE GENERAL CONCEPTION

It is a well-attested fact that the integrity of memorized material at a given time after memorization is a function of the kind and amount of psychological activity that has taken place between the original memorization and the attempted reinstatement. But, however substantially supported this generalization may be, one must admit that it is not customarily put forward in this form. We read of *retroactive inhibition*, an instance in which the interpolation of some activity between memorization and reinstatement reduces the integrity of that which has been memorized. We read of *fixation through repetition*, an instance in which interpolated activity increases the integrity of that which has been memorized—or at least retards its decay. Thus, we think of a retroactive inhibition, on the one hand, and of a retroactive facilitation on the other. Between them we think of a normal retentiveness which is usually taken to be that obtained when the interpolated activity is relatively passive, as in some cases of revery. No long discussion is required to show how arbitrary must be any such notion as that of normal retention. If normality (the condition of zero inhibition and zero facilitation) be thrown overboard, we shall have put our facts into a presumably continuous series of retention values, extending from that small retentivity, when the interpolated activity is the most unfavorable, to that great retentivity, when the interpolated activity is the most favorable. In this simplification, what is ordinarily called improvement due to repetition falls into quantifiable relationship with retroactive interference or inhibition. From this point of view there emerge new experimental tasks. We shall be tempted to vary the nature of interpolated activity quantitatively and continuously in order to produce as wide as possible a range of retention values; and we shall be approaching

[1]The experiments to be reported here were carried out in the Psychological Laboratory of the University of Chicago during the spring and summer of 1926. I am grateful to Professor Arthur G. Bills and to Mr. Clarence W. Brown, at that time assistants in the laboratory, for aid in the arrangement of the schedules of the *S*s and in the tabulation of data.

at once such seemingly discrete problems as that of the favorable effects of rehearsal and that of the unfavorable effects of certain types of confusion.[2]

Experiment may some day force us to the opinion that the rapid disintegration brought about by some forms of interpolated activity and the slow disintegration brought about by other forms of interpolation (for example, where interpolation is simply a continuation of the original memorization) are qualitatively discrete phenomena which cannot be placed as values within the same continuous function. But values which, from an empirical standpoint, do lie within the same function should not be placed in two separate functions unless there is positive evidence of a qualitative separation.

## The Special Problem

This relatively abstract prologue prepares the way for the special experimental problem with which the present paper is concerned. Of all the modes of variation that interpolated activity can manifest there is none more important in influence upon retention than the degree of similarity of that interpolated activity to the original memorization. According to current generalization, where interpolation and original memorization are identical, or almost identical, we get a high retention value (unless, of course, a 'fatigue' effect enters). Where interpolation and original memorization are similar in some intermediate degree we get a much lower retention value. Where the two activities, interpolation and original memorization, are very unlike we get a retention value somewhere between those obtained under the first two conditions. The general law would seem, then, to be something like this: *As similarity between interpolation and original*

[2]This attempt to treat retroactive facilitation and retroactive inhibition as representing simply higher and lower values within the single variable, retention, is not exactly new. L. W. Webb (Transfer of training and retroaction, *Psychol. Monog.*, 24, 1917, no. 104, 1-90) worked in terms of the more general conception of retroaction in his studies of the retention of maze habits in animals. Shortly after this the present writer (Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 28, 1920, no. 128, 1-57) pointed out the same continuity in the case of human memory, and also the difficulty of deciding upon the meaning of normal retention, *i.e.* the point where facilitation has just left off and inhibition just barely not begun. E. B. Skaggs, the author of the most important recent research in this field (Further studies in retroactive inhibition, *Psychol. Monog.*, 34, 1925, no. 161, 1-60), accepted the logic of the conception of continuity, though later (The concept of retroactive inhibition, *Psychol. Rev.*, 33, 1926, 237-244) he defended the original theory of retroactive inhibition expressed by G. E. Müller and A. Pilzecker (Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol.*, Erg. Bd., 1, 1900, 1-300) which tended to regard retroactive inhibition as a qualitatively unique phenomenon.

*memorization is reduced from near identity, retention falls away to a minimum and then rises again, but with decreasing similarity it never reaches the level obtaining with maximum similarity.* The presumable situation may be shown graphically as in Fig. 1. Skaggs (Fig. 2)

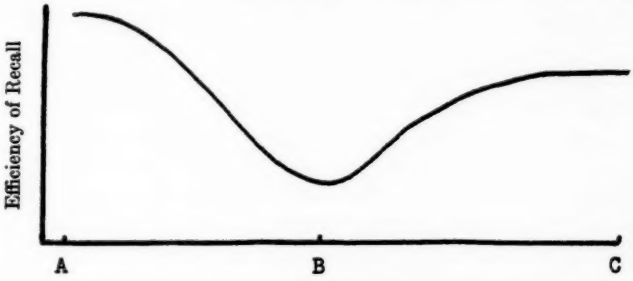


FIG. 1. SIMILARITY AND EFFICIENCY OF RECALL

At A, where similarity between interpolated activity and original memorization is at a maximum (where they are as nearly as possible identical), recall is at its highest level of efficiency. As this similarity approaches its minimum at C it passes through an intermediate degree, B, where recall is exceedingly inefficient. After this point is passed, there is an increase in efficiency of recall, but this increase does not return to the level obtaining at A.

seems to feel that two functions are required to show the same facts. One represents the fall of the reënforcement effect with decreasing similarity and the other represents the successive rise and fall of the inhibition effect with decreasing similarity. As we have indicated, we believe that such a procedure unduly complicates the facts. It implies that we know much more than is contained in our experimental results.

The law represented by our verbal and graphical statements certainly seems to be reasonable. But it is quite proper to look into the nature of the facts upon the basis of which we judge the law to be sound. When two sets of memorized material are *fairly* similar, we know from casual experience that interference effects frequently occur which do not occur when the materials are *very* similar or even when they are very dissimilar.

We also have experimental results bearing on the case. The writer utilized a variety of interpolated activities between the mem-

orization and recall of series of numbers.[3] Where the interpolated activities could be ranked according to their similarity to the original learning, the recall values fell in the inverse rank order. That is to say, the greater the similarity, the less efficient the recall. Here is an example: memorization of a set of 8 four-place numbers was followed by memorization of another set of such numbers or by memorization of a column of 32 single digits or by memorization of poetry. The rank order according to similarity is here perfectly definite and so were the recalls attained after these different types of interpolation.

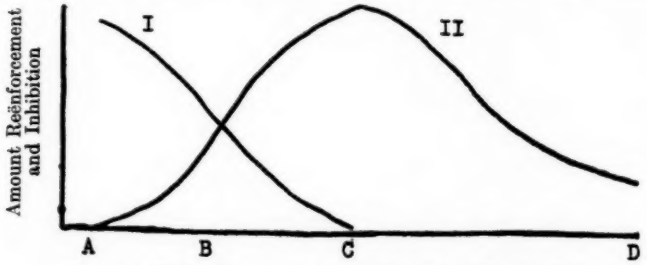


FIG. 2. SIMILARITY AND THE RETROACTIVE EFFECTS
(After Skaggs)

"Beginning at A where learning and work (interpolated activity) are identical, as we go to the right there is greater and greater dissimilarity until at D the two are as dissimilar in content and method as is possible. At A inhibition is at a minimum and reënforcement at a maximum (mere repetition); at C the situation is reversed. At D the inhibition curve has fallen but never to the original minimum." (Skaggs, *op. cit.*, 32).

It is evident, however, that this is far from a clear-cut demonstration of the theoretical law shown in Fig. 1. In the first place the experimental results show increasing recall with decreasing similarity, but that is only one phase of the presumable relationship. Of course, one can say that, with the interpolated activity consisting of further study of a list identical with the original, there would surely have been a high recall value and that, if such identical interpolation had been used, we could hardly have obtained anything other than the type of inversion represented by the theoretical law. Nevertheless, what we would surely have obtained and what we did obtain are different matters. The second limitation of the experiments in question is

[3]E. S. Robinson, *Psychol. Monog.*, 28, 1920, no. 128, Chap. II.

that they were quantitative in only the roughest sort of way. Although the interpolated activities could be ranked in definite order according to their similarity to the original memorization, they could only be *ranked.*

Skaggs' results are open to similar comment.[4] He demonstrated clear relationships between recall and the similarity factor, but he could in no case do more than rank his degrees of similarity. He also failed to demonstrate, within a single experiment, the inversion present in the theoretical function. In one instance, however, where the original memorization dealt with arrangements of men on a chess board and where the interpolated conditions were relatively slight modifications of the same activity, he did secure a positive relationship between the similarity factor and recall.

We may conclude, then, that the theoretical law of Fig. 1 has lacked anything like a complete experimental demonstration. By putting together casual impressions about memory in everyday life and a few experiments of an essentially qualitative sort, the presumptive law can be made to appear reasonable. But it is exactly because the probable state of affairs is so resistant to precise experimental demonstration that the problem deserves serious attention. In those cases where our rough generalizations seem especially obvious, and yet peculiarly resistant to experimental control, there is often a need for vigorous revision.

In our own thinking about this situation we first became suspicious of the universality of the generalization in question out of consideration of the nature of *similarity.* As long as we are talking in gross terms we are able to understand each other when we mention similarity. We should all agree that, from most standpoints, memorizing a second set of 8 four-place numbers is more like memorizing a first set of such numbers than memorizing twenty lines of poetry is like memorizing numbers. But with any attempt to say how much more similar are the members of one pair of activities than are those of another we get into difficulties, because we cannot describe the units of similarity in terms of which our judgments are made. Naturally we have such units, but it is quite certain that they are so heterogeneous in kind and so unequated in magnitude that they become almost meaningless if we attempt to take them out of the rough approximations of everyday thought.

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[4] *Psychol. Monog.*, 34, 1925, no. 161, 25-34, 49.

Since this is the case, it seems utterly hopeless to give quantitative meaning to a variable which is fundamentally qualitative and, what is more important, fundamentally heterogeneous. It also becomes hopeless to attempt to demonstrate with precision any law that is supposed to rest upon the possibility of that variable being simply defined and represented by some one dimension. But these sad thoughts by no means imply that the experimental ideal should be given up completely. What they do imply is the need for a simplification of the problem, even if its original nature be altered by the process. Granted that *similarity*, as the term is ordinarily used, cannot be summed up in a single dimension of variability, there may be simpler factors within the concept of similarity which can be rescued and pressed into service. In another connection, *i.e.* in respect to the relationship between similarity and degree of transfer of training, this possibility has long since been recognized. The procedure in that analysis is not, however, one that turned out well for the problem in question, nor is it one that can be recommended to clarify the present situation. All similarity was boiled down to homogeneity by inference regarding identity and disparity of neural elements involved. This conception would be perfect if one could select activities of known neurological constitution, give them definite positions within a single dimension by virtue of neurones in common, and then plot against this arrangement values representing amount of transfer from each to some standard activity. That this cannot be done even approximately is obvious. It should be equally obvious that an arrangement of activities in terms of the presence within them of common neural elements must be even less meaningful and definite than their arrangement according to the original, homely category of similarity. We are forced, therefore, to search in another direction.

It is only natural that we should turn back to analysis in terms of conventional psychological categories. Why not arrange activities according to the degrees to which they are memorial or the reverse (whatever that may be)? Why not arrange them according to the degrees to which they are imaginal? The objections are well known and one should, perhaps, be kind enough not to mention such a preposterous program. We have no units of memorialness, no units of imaginativeness. Yet such is the present strength of the tendency among us to give functional unity to the old faculties that there is some justification, even as we pass them by, for making explicit note of their uselessness for our present purpose.

### Experimental Possibilities

In our first experimental attack upon this problem we attempted to overcome the difficulties involved by utilizing a number of tasks of memorization each of which could be described in terms of a simple physical magnitude. *S* was presented with a line of a given length. An interpolated period followed during which a second line was presented. *S* reproduced the first line by drawing immediately after he had seen the second and, after that, reproduced the second. (The second was reproduced in order that *S* would not neglect paying attention to the interpolated line upon later repetitions of the experiment.) Every *S* went through these conditions on numerous occasions so that there finally accumulated considerable data showing the influence of the length of the interpolated line upon the memory for an original line of a given length. (Several standards were used.) Suffice it to say that there was no discernable relationship between retention and similarity of interpolation to original learning. In fact the retention of such a simple sensory fact as the length of a line seems to be peculiarly resistant to retroaction.

We next proceeded to a method that had early occurred to us, but not at first in feasible form. We thought of varying the relationship between an original and an interpolated list of nonsense syllables by having identical lists, lists alike except for one syllable in each, lists alike except for two syllables in each, and so on to the condition under which the lists contained no common syllables. But the execution of such a plan offered large practical difficulties. If the lists were each of 6 syllables, the common syllables would have to occur in all 6 positions. Otherwise the results would bear no more upon the degree of identity (similarity) of list to list than upon the particular locations at which the identity happened to occur. As a matter of fact, 64 experimental arrangements would have to be employed to cover in any adequate fashion the range from no syllables in common to 6 syllables in common. Anyone familiar with the time consumed by one *S* in learning one list, or pair of lists, of nonsense syllables will readily see that such an experimental program as this would not be feasible with ordinary resources of time.

Our search for a method that would permit a fairly wide range of experimental conditions without demanding colossal patience on the part of *S* and *E* brought us next to the memory-span experiment. It is the very principle of this procedure that there be a quick exposure of material and a prompt recall. The impression sought is a light one and the recall required gives little opportunity for anything but the simplest and most automatic type of response. Because of these features of the method it is usually assumed possible and proper to present to an *S* during a given experimental sitting several sets of numbers, consonants, or common objects. One need not assume an absence of all interference from list to list; it is sufficient if one assume that, after the first few sets of material have been exposed there will be no sudden or radical alteration in general susceptibility for this type of impression within the experimental sitting in question. Special precautions that we adopted will be mentioned later.

### Experiment 1

Since the memory-span method requires recall immediately after impression and without the *S* having time for 'thought,' our

first problem was that of reconciling this requirement with the interpolated period that must appear in any experiment on retroaction. The plan adopted was as follows.

We decided to begin our study with an original list of 4 consonants and an interpolated list of 4 consonants. These two lists were to be presented to the *S* as a continuous list of 8 consonants. At the conclusion of the exposure of the eighth character the *S* was to write his recall in the order in which the characters had appeared. He was not to retrace his steps in that recall. From the standpoint of results the presented material and the recalled material could be divided into original list and interpolated list exactly as they are in more conventional forms of the retroaction experiment. The main difference is that the *S* received his impressions with a less well-defined distinction between the original and the interpolated material. After a certain amount of experience with the conditions of experimentation a distinction was sure to grow in the *S*'s mind because of the fact that items within the first half of the total list often were repeated in the second half, while repetitions within the first half or within the second half were totally lacking. This point is interesting enough and its importance might be worth experimental examination. It was not crucial, however, for our present purpose.[5]

Exposure was to vision and each consonant was in view for 0.5 sec. Five final values were sought; recall for lists containing no consonants in common, recall with one consonant common to the two lists, recall with 2 in common, recall with 3 in common, recall with 4 in common. It was necessary to multiply the

| *Degree of Identity* | *Arrangement of Items* | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| None in common | a | b | c | d | e | f | g | h |
| One in common | *a* | b | c | d | *a* | f | g | h |
| | a | *b* | c | d | e | *b* | g | h |
| | a | b | *c* | d | e | f | *c* | h |
| | a | b | c | *d* | e | f | g | *d* |
| Two in common | *a* | *b* | c | d | *a* | *b* | g | h |
| | *a* | b | *c* | d | *a* | f | *c* | h |
| | *a* | b | c | *d* | *a* | f | g | *d* |
| | a | *b* | *c* | d | e | *b* | *c* | h |
| | a | *b* | c | *d* | e | *b* | g | *d* |
| | a | b | *c* | *d* | e | f | *c* | *d* |
| Three in common | *a* | *b* | *c* | d | *a* | *b* | *c* | h |
| | *a* | b | *c* | *d* | *a* | f | *c* | *d* |
| | a | *b* | *c* | *d* | e | *b* | *c* | *d* |
| | *a* | *b* | c | *d* | *a* | *b* | g | *d* |
| Four in common | *a* | *b* | *c* | *d* | *a* | *b* | *c* | *d* |

[5]A critic of a verbal report of our method assumed that the conception of retroaction contains in it an assumption that the validity of experimentation in this domain is connected with the presence in the *S* of a clear recognition of whether, or where, the *E* draws a line between original memorization and interpolated activity. That such a distinction was present in the early experiments of Müller and Pilzecker no one can deny. But those investigators, themselves, were certainly not concerned with that distinction, nor are we attempting to reproduce their work.

individual conditions of experimentation to such a point that the positional factor would be compensated for. Therefore, 16 conditions were required and they may be represented by the scheme above (p. 304) in which the successive positions are represented alphabetically, except to the degree to which repetitions occur in which case the situation is represented by a repetition of the appropriate earlier letter. It should be remembered that the actual experiment utilized only consonants.

The above arrangements do not cover all of the cases of one, two, three, and four items in common in lists of four items. There are the instances in which the first item in the first list is identical with the second, the third, or the fourth item in the second list. There are the instances in which the first and second items in one list are identical with the first and third, or with the third and fourth, items in the other list. Should the similarity-retroaction relation be determined where such partial identity, as exists, exists in corresponding positions or where it exists in any positions? We have no real choice, because we believe that both sets of determinations are legitimate and desirable. In the interests of simplicity, however, the effects of variable degrees of identity should receive separate investigation under these two sets of circumstances, *i.e.* where the positional factor is constant, as in our experiment, and where the positional factor is variable. There are two reasons why we chose to investigate the identity factor with position constant. In the first place, this task is the easier. The sixteen conditions make up a formidable enough experiment. But the other problem would demand many more conditions. In the second place, the problem with the positional factor constant seemed to us the simpler of the two in conception and, since both awaited investigation, we were naturally attracted in the direction of this simplicity.

Twenty graduate students in a training course in experimental psychology acted alternately as *S* and *E*. At a given sitting one member of each of the ten pairs into which the total group was divided, began as *E* while his partner served as *S*. After the presentation and recall under one of the sixteen experimental conditions, *S* and *E* changed parts. During the course of a single sitting each person performed once under each of the experimental conditions. There were from 4 to 6 sittings a week according to the preferences of the pairs of workers. The experiment proper was preceded by two practice sittings.

In order to compensate for practice and fatigue effects and for the possibility of anticipating the nature of any condition by the *S*, the order in which a given person went through the conditions was varied from day to day. Partners, of course, never had the same order on the same day. In all, 16 orders were used, but these were not secured by the usual method of going through from condition 1 to 16, from 2 through 16 to 1, from 3 to 2 and so on. Such a method would have made it entirely too easy for these *S*s, who also knew the experiment from *E*'s side, to anticipate the conditions. Sixteen random orders were obtained by shuffling and dealing cards. Our results will be given in terms of the recall of the first four consonants. Raw scores have been converted into percentages of the best possible performance. In order that differences developing during the progress of the experiment might be detected, we have arbitrarily divided the 9 full repetitions of the experiment into successive thirds.

We have had to take account of the fact that certain conditions, *e.g.* that of one item in common, appeared in the program oftener than others. The conditions of no items and of four items in common occur once each and that furnishes a base to which the others are made equivalent by division. The record made under the condition of one item in common had to be divided by four to be comparable with that made under the condition of four in common. The results for the condition of two in common had to be divided by 6. Similar corrections were applied wherever required. The results for a single cycle of the experiment are on the basis of three performances for every *S* under each experimental condition. That is, n = 60.

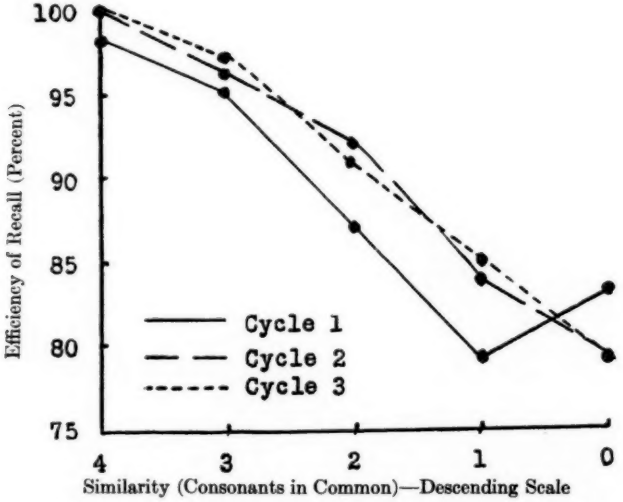


FIG. 3. RESULTS OF EXPERIMENT 1 BY INDIVIDUAL CYCLES
Visual presentation

*Results.* In Fig. 3 are the results of Experiment 1. Only in the first cycle do we find the inversion that is supposed to exist in the relationship between similarity and retroaction. For the meaning of the similarity factor as it exists in this experiment there seem to be definite limits to the inevitableness of any inversion of the first order. This is not because, after the first cycle, the *S*s anticipated the experimental conditions as they occurred. The situation was far too complex for that. It would seem, however, to be related to some general 'practice' factor. That, in itself, is highly important. So far as we know no one has yet suggested that the theoretical function (Fig. 1) holds only for situations that are very novel to the *S*.

It would be surprising if someone did not suggest that the failure of our experiment to secure the expected inversion after the first cycle was due to the fact that we utilized too narrow a range of similarity. Our reply would be that we worked from complete identity to complete distinction. The counter-reply might be that, even where there were no consonants identical in the two lists, there was still considerable similarity between them and that the inversion in question may occur at a degree of similarity smaller than any within our experiment. Arguments of this order, however, would get us involved in all of the difficulties of the rough-and-ready definitions of similarity. We have already dwelt upon them at length and upon the necessity of working in terms of restricted and sharply defined types of similarity. If this is done, similarity, in our experiment, can refer only to the number of consonants common to two lists. As long as we mean this and nothing more than this it is legitimate to say that we have varied the similarity under control from its minimum to its maximum degree. Having done this, we have uncovered a situation in which there is a continuous increase in recall values as we proceed from complete identity to complete distinction. Even if this be exceptional, it still is important. If all the definable instances of similarity variation were brought under control, it might be found that the majority of the functions obtained were of the general type shown in Fig. 1. Yet proof that this would be the case is lacking. The results of our first cycle show that the function can contain a first order inversion. The results of our second and third cycles, on the other hand, show that such inversions can be lacking. Thus we have thrown open the exceedingly interesting and important and experimentable problem as to the circumstances under which the inversion appears and the circumstances under which it is lacking.

## Experiment 2

Our second experiment was a repetition of the first with the one important exception that the material was presented to ear instead of to eye.

The consonants were read at the rate of 2 a second and with as little rhythm as possible. The same *S*s and *E*s took part, the same conditions were employed, and the same number of observations taken. The results were given the same treatment and, in Fig. 4, are analogously presented.

*Results.* Again the first cycle shows a first order inversion of our function and again the poorest recall in that first cycle occurs with

one item in common. But, as in the earlier experiment, the inversion has disappeared in the second and third cycles. There is some evidence here that a factor of general familiarity with the reception of material under these special conditions is the important element in eliminating the inversion. If the determining influence were a matter of insight into the scheme of item arrangements we should hardly have expected the first order inversion to appear again at the begin-

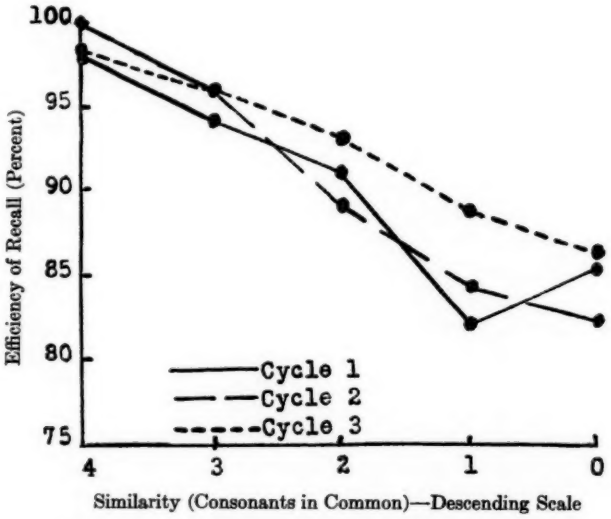


FIG. 4. RESULTS OF EXPERIMENT 2 BY INDIVIDUAL CYCLES
Auditory presentation

ning of the second experiment. Just how unfamiliarity with the mode of presentation should produce this inversion, which further practice obliterates, is not at all clear.

The high degree of correspondence between the results from the visual and the auditory presentations is really remarkable (cf. Fig. 5). A somewhat better record was made with the auditory presentation, but this can be accounted for in terms of the fact that the *Ss* came to the auditory experiment after having had considerable practice at visual reception of the same type of material.

## EXPERIMENT 3

The tasks of memorization required by our first two experiments were relatively easy. The most confusing conditions showed a recall value of 78% of perfection. Under the best circumstances, *i.e.* with identical lists, the recall value was about 97–100%. Now it seemed perfectly possible that a relationship between the similarity factor and retroaction, which might hold with a relatively easy task, would not hold for a task of greater difficulty. Because of this consideration

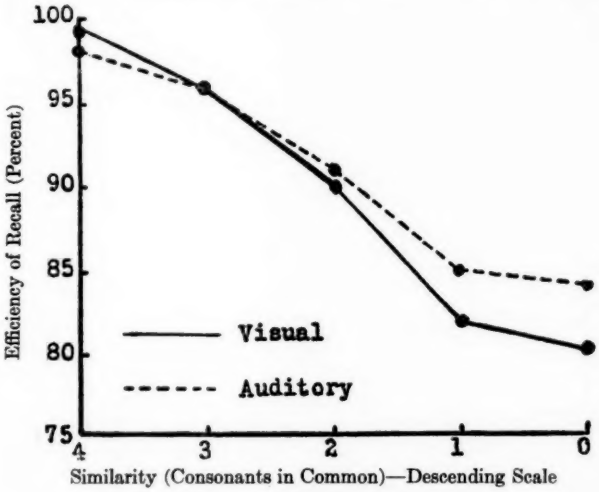


FIG. 5. RESULTS OF EXPERIMENTS 1 AND 2
Averages for entire experiments

we next proceeded to an experiment in which 12 consonants were to be presented in immediate succession and in which the division, from the standpoint of our analysis, was to be between an original list of 6 items and an immediately following interpolated list of 6 items. The plan was to have from 0 to 6 consonants common to the lists.

We handled the position factor as we had in the earlier work. That is to say, the condition of one item in common occurred with that item in each of the 6 places. Similarly the arrangements for the other degrees of identity or similarity were carried out in a manner analogous to that followed in Experiments 1 and 2. It is hardly necessary to detail the arrangements as we did for Experiment 1. Suffice it to say that in order to compensate for the order factor we had to use 64 separate types of arrangement. The condition of three elements in common alone

demanded 20 arrangements. In this as in the earlier experiments, we made our determinations only for the cases where the common elements appear in corresponding positions in the two lists.

Sixteen *S*s served in Experiment 3. None of them had served in Experiments 1 or 2, but they were persons of similar training and laboratory experience. After two sittings devoted to familiarizing themselves with the general conditions of

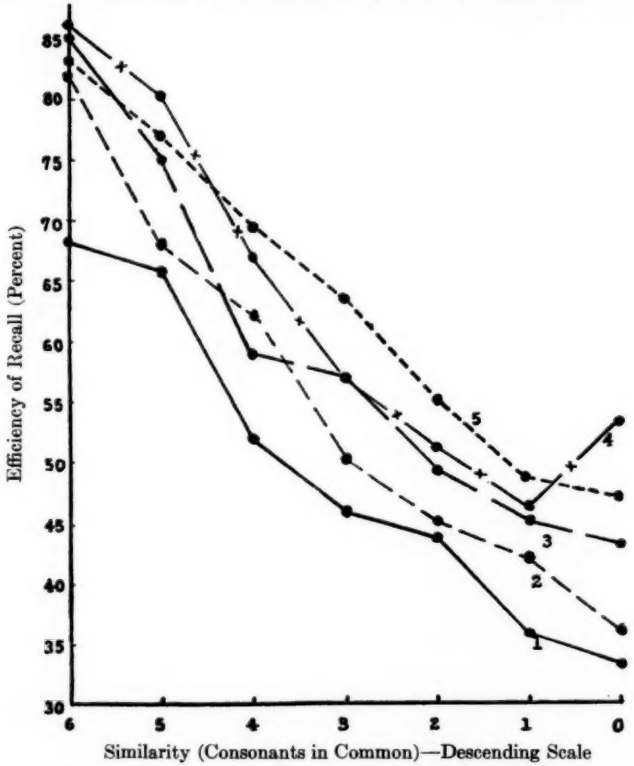


FIG. 6. RESULTS FOR EXPERIMENT 3 BY INDIVIDUAL CYCLES
Numbers on curves identify the successive cycles of experiment

experimentation, the regular work began. The auditory method of presentation was utilized because it was simpler and because the earlier results had indicated no great difference between it and the visual method so far as this problem was concerned. Again workers alternated as *S* and *E*. Including different points of starting and different orderings of the 64 individual conditions there were 12 chance orders of procedure through the entire set of conditions. This precaution

was certainly sufficient to counterbalance any important practice or fatigue influences upon the relative standing of the principal conditions. The best test of its efficacy will lie in the regularity with which obtained values are related to the controlled variable. This will be found to be quite satisfactory.

On any given experiment day one *S* went through only 16 of the 64 conditions of experimentation. Four experiment days were thus required to complete a cycle, *i.e.* a period containing one performance by every *S* for every individual condition.

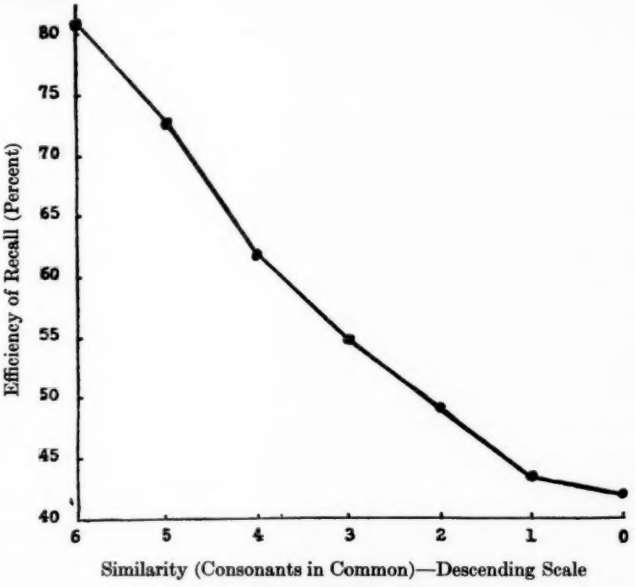


FIG. 7. RESULTS FOR EXPERIMENT 3
Average for entire experiment

*Results.* The results of this experiment are given by individual cycles in Fig. 6. Amounts of correct recall of the first list of 6 consonants are plotted against the number of items common to the original and the interpolated list. Where one degree of identity was represented by a number of individual conditions, the recall value has, of course, been reduced to the same basis as that of the o identity, which had only one representation per cycle.

The curves show that we achieved our purpose in setting a much more difficult memory task. The recalls range from about 87% to about 33%. Yet we still have a conspicuous absence of the inversion

that has been confidently assumed to mark the relationship between similarity and retroaction. True, such an inversion occurs in the curve for the fourth cycle, but it has everything about it to suggest statistical accident. The average values for the entire experiment (Fig. 7) clearly give the general picture of the relationship obtained. Four out of five of the individual curves conform to the general relationship shown by the average. This relationship is also that obtained throughout the greater portion of the two earlier experiments.

## Conclusions

Having already said so much by way of interpretation, we can now draw our conclusions very briefly. Although the relationship between similarity and retroaction (or recall) presented in Fig. 1 seems on a basis of general impression to be the typical one, it has lacked experimental demonstration. We have attempted such a demonstration. But we have been unable to work with similarity in general or on the average. We have had to confine ourselves to experimentable conceptions of this mode of variation. In this limitation we have had the comforting thought that only when conceived as limited and definable can similarity ever have any quantitative meaning. Our results have failed to demonstrate the assumed generality of the relationship expressed by what we have been calling the theoretical function. The inversion of the first order, which is its marked peculiarity, has been more often absent than present in the functions experimentally obtained. This is true in spite of the fact that we have worked with hard as well as with easy tasks of memorization. Now it would be foolish to claim proof that the theoretical relationship is not that which would be obtained from the majority of possible experimental investigations of the question. What we can claim proof of is this: that under such circumstances as we have worked we have shown that the theoretical relationship does not hold as often as a simpler one. In other words, however many may be the conditions under which there is a first order inversion in the debated function, there are demonstrable conditions under which such inversion is absent.

# REDUCTION OF ARTICULATION

By CHARLES H. JUDD, University of Chicago

There are several lines of evidence which point to the conclusion that the reduction of movements of articulation is essential to the most highly developed uses of language. It has been shown in all recent investigations of reading that silent reading not only is more rapid than oral reading but is also likely to be correlated with a higher degree of comprehension of meaning. Lip movements during silent reading and movements in the larynx are signs of immaturity. The schools have been led by such considerations as these to devote increasing attention to the early cultivation of forms of reading in which the movements of articulation are reduced to a minimum. A second line of evidence appears in the fact that the effort to think in a foreign language or to read passages in a foreign language is accompanied by more noticeable movements of articulation than is thinking or reading in the vernacular. In general, children are observed to do much of their thinking out loud while adults have reduced the movements of articulation to a point where they are for the most part too slight to be observed. When one becomes excited in one's thinking or deeply absorbed in a train of ideas, the accompanying neural processes sometimes become strong enough to produce observable contractions of the vocal muscles or of the muscles of the face. These phenomena are more often observed in persons of the lower levels of mental development.

An opportunity to investigate the problem of reduced articulation under very favorable conditions presented itself in the course of a series of experiments which the author of this paper was conducting in the effort to analyze some of the simpler stages of number experience.[1] It was found in the experiments with both adults and children that all counting is in some degree accompanied by articulation. Children not uncommonly make movements with parts of the body not directly involved in pronouncing the number names. They nod the head or tap with the fingers or with the foot. Evidence was found which shows that counting is a motor process and usually

[1] C. H. Judd, *Psychological Analysis of the Fundamentals of Arithmetic*, 1927.

involves more movements of articulation than does ordinary thinking in words. A striking case of articulation was found in the fact that counting from one to twenty required much more than twice as long as did counting from one to ten. This is doubtless to be explained by the elaborate number names beyond ten.

The very noticeable movements of articulation which are exhibited in counting, even by trained adults, suggested the possibility of collecting evidence, by means of experiments in this field, for the purpose of throwing light on the whole matter of the reduction of articulation as a phase of mental development.

TABLE I

Average Number of Seconds Required by 5 *S*s in Counting Various Series

| Method of Counting | Numbers Counted | *S* | | | | |
|---|---|---|---|---|---|---|
| | | M | N | O | P | Q |
| Oral | one to ten | 1.2 | 1.4 | 1.1 | 1.1 | 1.6 |
| | one to twenty | 3.4 | 3.7 | 3.0 | 3.2 | 4.1 |
| | eleven to twenty | 2.4 | 2.9 | 1.9 | 2.0 | 2.7 |
| | twenty-one to thirty | 1.9 | 1.5 | 1.3 | 1.6 | 2.2 |
| | one to thirty | 5.4 | 4.8 | 4.2 | 5.1 | 5.7 |
| Silent | one to ten | 1.3 | 1.2 | 0.9 | 1.4 | 1.3 |
| | one to twenty | 3.0 | 3.2 | 2.4 | 3.1 | 3.6 |
| | eleven to twenty | 1.8 | 1.9 | 1.6 | 1.9 | 2.4 |
| | twenty-one to thirty | 1.6 | 1.2 | 1.3 | 1.7 | 2.1 |
| | one to thirty | 4.2 | 4.2 | 3.3 | 5.0 | 5.3 |

Five adult subjects were employed in the series of experiments to be reported in this paper. Some of the results secured from these subjects have been used in another connection and one of the tables reported in an earlier report will be repeated here.

Table I[2] reports measurements of the time required by 5 adult *S*s to count under various conditions which are recorded in the first two columns of the table. The method of the experiment was to require each *S* to count and at the same time to record by the use of a reaction-key the time required for each series. In order to reduce so far as possible any error introduced by the reaction-time of the hand in manipulating the reaction-key, *S* was required to repeat each series a number of times without stopping between the series. Thus when counting orally from one to ten, *S* pressed the key when he said one and lifted his hand when he reached ten. He repeated

[2] *Op. cit.*, 33.

the series from one to ten without interrupting his counting and pressed down the key a second time when he arrived at the second ten. In this way a series of measurements were secured by reactions at the beginning and end of each series. The counting in each case continued until more than an aggregate of a hundred had been counted.

Table I shows several important facts. Counting from eleven to twenty is, as was noted before, much more difficult than counting from one to ten even when *S* is counting silently.

Counting from twenty-one to thirty is a very different process in different individuals. Two introspective observations of the *Ss* are important in explaining the facts presented in the table with regard to counting from twenty-one to thirty. P was clearly conscious of the full emphasis on the twenty in twenty-one, twenty-two and so on. N deliberately omits the twenty in both oral and silent counting. He counts as follows: twenty, one, two, three, and so on until he reaches thirty.

TABLE II

Av. No. of Numbers Counted by Various *Ss* in 1 Sec. under Various Conditions

| Method of Counting | Numbers Counted | *S* | | | | |
|---|---|---|---|---|---|---|
| | | M | N | O | P | Q |
| Oral | one to ten | 8.3 | 7.1 | 9.1 | 9.1 | 6.3 |
| | one to twenty | 5.9 | 5.4 | 6.7 | 6.3 | 4.9 |
| | eleven to twenty | 4.2 | 3.4 | 5.3 | 5.0 | 3.7 |
| | twenty-one to thirty | 5.3 | 6.7 | 7.7 | 6.3 | 4.5 |
| | one to thirty | 5.6 | 6.3 | 7.1 | 5.9 | 5.3 |
| Silent | one to ten | 7.7 | 8.3 | 11.1 | 7.1 | 7.7 |
| | one to twenty | 6.6 | 6.3 | 8.3 | 6.5 | 5.6 |
| | eleven to twenty | 5.6 | 5.3 | 6.3 | 5.3 | 4.2 |
| | twenty-one to thirty | 6.3 | 8.3 | 7.7 | 5.9 | 4.8 |
| | one to thirty | 7.1 | 7.1 | 9.1 | 6.0 | 5.7 |

Table II is a different arrangement of the facts in Table I and renders comparison easier. An examination of Table II brings out the striking fact that in almost every instance silent counting is more successful than the corresponding case of oral counting. There is evidently some economy in the partial reduction in articulation which occurs for all *Ss* in silent counting. Furthermore, the degree to which various *Ss* achieve this economy is quite different. M, N, and O are in general more successful in silent counting in comparison with their own records in oral counting than are P and Q.

Further indications of the existence of marked individual differences are uncovered by the comparisons made in Table III. Table III represents an effort to isolate from the two cases in which the *S*s counted from eleven to twenty in connection with other numbers this most difficult decade. The first and fourth horizontal lines of this table are taken directly from Table II. The other four lines are

TABLE III

Av. No. of Numbers Counted in 1 Sec. by Various *S*s under Different Conditions of Counting from Eleven to Twenty

| Method of Counting | Numbers Counted | *S* | | | | |
|---|---|---|---|---|---|---|
| | | M | N | O | P | Q |
| Oral | eleven to twenty | 4.2 | 3.4 | 5.3 | 5.0 | 3.7 |
| | one to twenty—last ten computed | 4.5 | 4.3 | 5.3 | 4.8 | 4.0 |
| | one to thirty—middle ten computed | 4.3 | 5.3 | 5.6 | 4.2 | 5.3 |
| Silent | eleven to twenty | 5.6 | 5.3 | 6.3 | 5.3 | 4.2 |
| | one to twenty—last ten computed | 5.9 | 5.0 | 6.7 | 5.9 | 4.3 |
| | one to thirty—middle ten computed | 7.7 | 5.6 | 9.1 | 5.3 | 5.3 |

calculated. If the mental and motor processes involved in counting the various decades could be assumed to be fairly uniform processes in all connections in which they appear, Table III ought to show in each vertical column complete uniformity of achievement. The table shows in a very striking way that for some *S*s eleven to twenty when counted as part of a series including other numbers is very different from the same numbers when counted by merely repeating the single decade. It should be borne in mind that the figures reported in Table I are averages and not single cases. The fluctuations which appear at a number of points in Table III show that habits of articulation are very complicated and highly individualized forms of behavior.

TABLE IV

Av. No. of Numbers Counted in 1 Sec. by Various *S*s under Different Conditions of Counting from Twenty-one to Thirty

| Method of Counting | Numbers Counted | *S* | | | | |
|---|---|---|---|---|---|---|
| | | M | N | O | P | Q |
| Oral | twenty-one to thirty | 5.3 | 6.7 | 7.7 | 6.3 | 4.5 |
| | one to thirty—last ten computed | 5.0 | 9.1 | 8.3 | 5.3 | 6.3 |
| Silent | twenty-one to thirty | 6.3 | 8.3 | 7.7 | 5.9 | 4.8 |
| | one to thirty—last ten computed | 8.3 | 10.0 | 11.1 | 5.3 | 5.9 |

Tables IV and V are arranged to permit comparisons of the same general type as that exhibited in Table III. Tables IV and V are striking in their revelation of a fundamental difference between M, N, and O on the one hand and P and Q on the other hand.

The difference between M, N, and O, and P and Q can be described in a summary way by saying that the first three have achieved more economy and therefore more efficiency in silent counting than have P and Q. This economy must be thought of as resulting from the reduction in some way of nonessential mental and neural processes. Arguing from the general fact that a major economy is achieved by

TABLE V

Calculated Numbers Counted as Last Twenty of Counting from One to Thirty under Various Conditions

| Methods of Calculation | *S* | | | | |
|---|---|---|---|---|---|
| | M | N | O | P | Q |
| A. Results when numbers counted orally from "eleven" to "twenty" are added to numbers counted from "twenty-one" to "thirty" | 4.7 | 4.5 | 6.3 | 5.6 | 4.1 |
| B. Results when numbers counted orally from "one" to "ten" are substracted from numbers counted from "one" to "thirty" | 4.8 | 5.9 | 6.5 | 5.0 | 4.9 |
| C. Calculation for silent counting similar to A above | 5.9 | 6.5 | 6.9 | 5.6 | 4.4 |
| D. Calculation for silent counting similar to B above | 6.9 | 6.7 | 8.3 | 5.6 | 5.0 |

silent counting as contrasted with oral counting, we may conclude tentatively that where there is a high degree of economy there is a corresponding reduction of articulation and where the economy is least the reduction of articulation movements is only partial.

Reference was made in an earlier paragraph to the special method adopted by N in counting the numbers in the decade of the twenties. This special device adopted by N is evidently a source of very great advantage to him whenever he has to deal with twenties. He does not seem to have any equally effective method of reducing the effort involved in counting other groups of numbers. A special trick such as that of omitting twenty is not effective in general. On the other hand, there is clear evidence that M and O have some kind of ability which is more generally effective, while P and Q are in

general much less effective. These clearly recognizable differences supply us with a basis for pursuing the investigation of the hypothesis formulated in an earlier paragraph that economy in counting is due to reduction of articulation movements and the attendant neural processes.

The ability of all the *S*s was further tested by requiring them to count series of sounds and flashes of light. The apparatus employed in this part of the experiment has been described elsewhere[3] and needs no description here beyond the statement that it provides a means of giving various series of taps by a sound-hammer separated by uniform intervals and capable of adjustment so that the rate and number of the sounds can be arranged according to the wishes of the experimenter. By means of the same apparatus a series of silent flashes of light in a Geissler tube can be given to *S*. In the earlier experiment with a large number of *S*s it was shown that ability to count flashes of light is in all cases less fully developed than is ability to count sounds. Marked individual differences were also shown to exist.

For the purposes of the present paper the results secured from the 5 *S*s—M, N, O, P, and Q—in counting sounds and flashes of light are important for two reasons. First, the tests show that the 5 *S*s are very different in ability. M and O are superior and Q is distinctly inferior. Secondly, the 5 *S*s are like all others tested in their relatively low ability to count flashes of light.[4]

Tables VI and VII exhibit the facts in detail. It should be kept in mind in examining these tables that the counting was in all cases silent counting.

It has been conclusively proved by the facts reported up to this point that there is a marked difference in the degree to which the *S*s examined have achieved efficiency in counting. It now remains to relate the results secured with numbers to a wholly different group of results secured with reading. If the reduction of articulation is a source of mental economy it is important that we determine whether a person who has achieved economy in one sphere, where reduction in articulation may be assumed to be the explanation, will be equally successful in achieving success in some other field, where the same explanation seems tenable.

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[3] *Op. cit.*, 19.

[4] In the experiments reported earlier any complications from after-images were shown to be eliminated. *Op. cit.*, 26.

It may be pointed out that we are here in a position to put to a test a matter which has often been discussed in theory. Does transfer of training take place in the sphere of methods of mental operation as well as in content of experience?

TABLE VI
Errors Made in Counting Series of Sounds (Counting Silently)

| Number of Sounds Given | Erroneous Numbers Reported by *S* | | | | |
|---|---|---|---|---|---|
| | M | N | O | P | Q |
| Six per second: | | | | | |
| 7 | — | — | — | — | 6 |
| 9 | — | 8 | — | — | — |
| 5 | — | — | — | — | — |
| 12 | — | 11 | — | — | 10 |
| 4 | — | — | — | — | — |
| 15 | — | 12 | — | — | 13 |
| Number of errors | 0 | 3 | 0 | 0 | 3 |
| Seven per second: | | | | | |
| 6 | — | — | — | — | 4 |
| 12 | 11 | 11 | — | 11 | 10 |
| 10 | — | 8 | — | — | 9 |
| 5 | — | — | — | — | — |
| 9 | — | 10 | — | — | 8 |
| Number of errors | 1 | 3 | 0 | 1 | 4 |
| Eight per second: | | | | | |
| 5 | — | 4 | — | 4 | 4 |
| 4 | — | 3 | — | — | 3 |
| 10 | 8 | 11 | 8 | — | 9 |
| 9 | 8 | 7 | — | 7 | 8 |
| Number of errors | 2 | 4 | 1 | 2 | 4 |
| Nine per second: | | | | | |
| 4 | — | 3 | — | 3 | 2 |
| 3 | — | 5 | — | — | 4 |
| 10 | 9 | 8 | 9 | 8 | 8 |
| 8 | 7 | 7 | 6 | 7 | 9 |
| Number of errors | 2 | 4 | 2 | 3 | 4 |

As a preliminary step to this second part of the inquiry the *S*s were arranged in an order corresponding roughly to their achievement of efficiency in all forms of silent counting. There is no possibility of establishing an absolute order of superiority, because at some points a given *S* falls behind and at others seems to exhibit unusual

success. Neglecting minor irregularities, it is possible to arrive at an arrangement which is certainly valid in its extremes and fairly satisfactory throughout. The order is that which has been maintained

TABLE VII

Errors Made in Counting Series of Flashes of Light
(Counting Silently)

| Number of Flashes of Light Given | Erroneous Numbers Reported by *S* | | | | |
|---|---|---|---|---|---|
| | M | N | O | P | Q |
| Three per second: | | | | | |
| 6 | — | — | — | — | 5 |
| 5 | — | — | — | — | — |
| 11 | 10 | 9 | — | — | 10 |
| 7 | — | — | — | — | 5 |
| 6 | — | 5 | — | — | — |
| Number of errors | 1 | 2 | 0 | 0 | 3 |
| Four per second: | | | | | |
| 4 | 3 | 3 | — | — | — |
| 7 | 6 | 5 | 6 | 5 | 5 |
| 5 | — | 4 | — | — | 4 |
| 9 | 7 | 8 | 8 | 7 | 7 |
| 6 | 5 | 4 | 5 | — | 5 |
| Number of errors | 4 | 5 | 3 | 2 | 4 |
| Five per second: | | | | | |
| 4 | 3 | — | 3 | — | 3 |
| 5 | 4 | 4 | 4 | 4 | 4 |
| 7 | 5 | 6 | 6 | 6 | 5 |
| 4 | 3 | 3 | 3 | — | 3 |
| 6 | 4 | 5 | 4 | 5 | 4 |
| Number of errors | 5 | 4 | 5 | 3 | 5 |
| Six per second: | | | | | |
| 4 | 3 | 3 | 3 | 3 | 3 |
| 6 | 4 | 5 | 4 | 4 | 4 |
| 5 | 4 | 3 | 4 | 4 | 4 |
| 7 | 5 | 6 | 6 | 8 | 5 |
| Number of errors | 4 | 4 | 4 | 4 | 4 |

throughout the tables. M is superior to the other *S*s in degree of economy effected and is consistent in exhibiting this economy except at a single point. That point is shown in Table I in the silent counting from one to ten. N shows only a moderate degree of economy but

is quite consistent. O and P are irregular and inferior to M and N. Of the two O seems to be somewhat superior. Q evidently stands at the lower limit of the group.

The 5 *S*s were tested in their ability to read orally and silently a number of simple newspaper articles which were of the same length and of approximately the same difficulty. The results are exhibited in Table VIII. The figures given in the last two columns at the right are indicative of the relative difficulty of the various passages. For our purposes the figures given in the horizontal line at the bottom of

TABLE VIII

Number of Seconds Required to Read Various Passages Orally and Silently

| Passages | *S* | | | | | | | | | | Average Time per Passage | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | M | | N | | O | | P | | Q | | | |
| | Oral | Silent | Oral | Silent | Oral | Silent | Oral | Silent | Oral | Silent | Oral | Silent |
| I | 74 | | | 29 | 67 | | | 44 | 75 | | 72.0 | 36.5 |
| II | | 18 | 65 | | | 28 | 62 | | | 49 | 63.5 | 31.6 |
| III | 68 | | | 30 | 60 | | | 47 | 71 | | 66.3 | 38.5 |
| IV | | 25 | 83 | | | 33 | 78 | | | 63 | 80.5 | 40.3 |
| V | 64 | | | 29 | 62 | | | 44 | 71 | | 65.0 | 36.5 |
| VI | | 25 | 79 | | | 34 | 76 | | | 63 | 77.5 | 46.3 |
| Average | 68.7 | 22.7 | 65.6 | 29.3 | 63.0 | 31.7 | 72.0 | 45.0 | 72.3 | 58.3 | | |

the table are the most significant. The time required by the various *S*s for silent reading of the passages makes possible a direct comparison of their efficiency in silent reading.

There is striking evidence that the *S*s who had achieved a high degree of economy in counting exhibited also a correspondingly high degree of economy in silent reading. M reads passages silently in about one-third of the time which he requires to read approximately equal passages orally, while Q, on exactly the same basis of comparison, economizes only a small fraction of the time.

In the reading test the *S*s fall into the order in which they were arranged on the basis of the number test. This makes it highly

probable that the hypothesis is valid that reduction of articulation movements is the source of economy in both cases.

The conclusion arrived at through the experiments contributes in an important way to the explanation of the learning process. Learning cannot be explained as a mere accumulation of fixed neural processes. Learning is a process of gradual adjustment in which certain primitive forms of response gradually merge into new and more economical forms of reaction. When a child first learns a word his response is emphatically a response of articulation. The other elements of motor response which accompany vocalization are relatively less important in the total complex of behavior than they will be as development goes forward. During the child's mastery of language and later in his mastery of reading and the interpretation of number, the purely vocal reaction becomes less and less prominent. Marked individual differences appear in these later stages of development because external control of the individual's reactions are less influential than they were in determining the forms of articulation. The responses of a highly organized individual are to subtle phases of meaning rather than to mere pronunciation. A number comes to mean a mere transition from one point to another in a series. A word stands for a response of acceptance or rejection or for a response of spatial adaptation. The way in which the transition is recorded in the individual's response mechanism will depend on an infinite number of internal chances. The only uniformity between the processes of development in different individuals will be in the negative fact that response to meaning is away from mere articulation. Reduction of articulation is therefore less significant in itself. It is significant as a correlate and symptom of the fact that an internal system of response is being developed which centers in reaction to meaning rather than in mere pronunciation.

# INDIVIDUAL DIFFERENCES IN REACTION TO THE WORD-IN-ITSELF

By JUNE E. DOWNEY, University of Wyoming

Word-consciousness is a much more sophisticated, highly specialized consciousness than is the usual speaking or reading or thinking consciousness; but it is the natural consciousness of the literary artist who in this respect may be sophisticated from the cradle. There may be an interest in the word for itself alone that is manifested in the earliest years so that the young poet or stylist is charmed by bare words; he would play with them as with fascinating toys.

I would emphasize the statement that one word used by itself is a much richer experience than the same word used in a sentence. The 'word' as a detached consciousness has a tendency to blossom into all manner of images, feelings, impulses. It is a focus of associations, often of exceeding richness. It is haloed with meaning. The same word in a sentence is held rigid in a given scheme of associations. It no longer possesses the youthful possibility of developing into a myriad wonders. It is sober and grown-up and become part of a system of somewhat stolid relationships.

Style swings between vivid word-consciousness and meaning or sentence-consciousness. Scientific style demands such transparency of meaning that each word shall be unambiguously limited by every other. Present-day efficiency demands in the workaday world a style so simple that he who runs may read. There is no time to wait for the meaning to be mediated by concrete imagery; the inner speech must be reduced to the barest flicker. Literary style is, in contrast, often enamoured of ambiguity, of double and triple implication of meaning. Words may achieve separate values. The single word or the phrase of a refrain may sparkle as a jewel in relief against the sober gold of the setting.

The present report summarizes the introspections of a number of psychologically trained subjects concerning their spontaneous reactions to isolated words. The instructions given to them were so planned as to restrict experience as far as possible to the word-in-itself, that is, to induce an attitude quite distinct from that of the usual experiment on association.

One hundred nouns and adjectives were typed separately on small squares of paper. These nouns and adjectives were chosen to cover a wide range of meaning, but care was taken to introduce into the list words that are ordinarily thought of as possessing poetic content, such words, for example, as the following: twilight, halcyon, drowsy, nightingale, mystic. Care was also taken to include in the list an approximately equal number of words beginning with the twenty-one consonants of the alphabet. Not all *S*s reacted to the whole list of one hundred words. The range was from fifty to one hundred words.

The *S* was instructed to assume a relaxed and passive attitude. As each word was presented, he was to become absorbed in it and then at a signal dictate to the *E* the experiences aroused by the word. A list of possible reactions were placed before him at the beginning of the experiment but no emphasis was placed on conformity to it. In fact, the *S*s gave many unanticipated reactions. The list included the following: Verbal Completions, Synonyms, Antonyms, Imagery of Any Sort, Affective and Emotional Reactions, Attitudes, Other Reactions.

Nine *S*s took the test and it is on their reports that the following discussion is based. A tabular summary is given of the main types of reaction. See Table I.

The investigation was carried on some years ago. If a similar investigation were planned today, an attempt would be made to modify methods of procedure so as to render them more objective. The results are, however, of value in that they make possible a comparison between reactions to isolated words and appreciation of the various aspects of color as reported by Bullough[1] or of tone as reported by Myers.[2] Literary esthetics is so unworked a field as to make even a meager contribution of some value. Study of esthetic responses by objective means is a development of the future.

Bullough recognized the following types of reaction to color: (1) the objective; (2) the physiological; (3) the associative; (4) the ascription of character.

The objective reaction to color consists in a preoccupation with such aspects of color as its purity or impurity of tone, its saturation, its luminosity, aspects which may be described in terms of the stimulus. An *S* with an objective attitude uppermost finds it somewhat absurd to speak of a single color as mediating or determining preference, much less emotion; he is somewhat disdainful of color-therapeutics. The physiological reaction is a subjective one, a preoccupation with the bodily or physiological effects of color. Colors are said to be warm or cold; heavy or light; soothing or depressing. Very strong color likes and dislikes are reported by such *S*s. For the associative type, color gets its emotional and preferential values from association with the things of the outer world. 'Blue' is beautiful as the sky-color; 'crimson' is reminiscent of blood and may for that reason be distasteful. Such *S*s refer to colors in terms of the objects so colored; thus, they speak of leaf-brown, corn-color, emerald-hues. The character type of reaction is, to Bullough's notion, a further development of the physiological and the associative. It consists in reading the subjective reaction back into the color itself; it objectifies the subjective aspect, so to speak. 'I am no

[1]E. Bullough, The 'perceptive problem' in the aesthetic appreciation of simple colour-combinations, *Brit. J. Psychol.*, 3, 1909-10, 406-447.

[2]C. S. Myers and C. W. Valentine, A study of the individual differences in attitude towards tones, *ibid.*, 7, 1914-15, 68-112; C. S. Myers, Individual differences in listening to music, *ibid.*, 13, 1922, 52-71.

longer excited by the color, rather it is itself exciting or soothing.' This attitude, this empathic projecting of feeling-content into an object, is one with which we are familiar from other reports on psychological esthetics.

Myers departs from Bullough's scheme in (1) the inclusion of conative experience with Bullough's 'physiological' aspect and use of 'intra-subjective' as the inclusive term, and (2) in extension of Bullough's 'objective' aspect to include consideration of the material "in regard to its use and meaning generally as an object."[3]

In order to compare the reactions to words with those obtained by Bullough and Myers to colors and tones, I have employed somewhat the same classificatory scheme as theirs, adopting Myers' term 'intra-subjective' and using the term 'personalization' to refer to the ascription of 'character' to a word. I have added 'symbolization' as a separate head. In all I have made use of five main heads for classificatory purposes: (I) Comments on the words as objects; (II) Associative reactions; (III) Intra-subjective reactions; (IV) Symbolizations; and (V) Personalizations. The various subheads under the main titles are self-explanatory. I have also commented on (VI) The total response.

It must be noted that words, even when used in isolation, are complex experiences. As bare stimuli, they are not merely visual but, because of their repetition in inner speech, vocal and auditory stimuli as well. They are, possibly, more charged with meaning than are simple colors and tones. For example, the word as a bare stimulus might be thought of in turn as (a) a visual pattern, (b) a vocal pattern, (c) an auditory pattern. If, instead of considering the word as a sensory stimulus, one may think of it mainly as meaning, one may distinguish the bare meaning from a meaning embodied by an image or attitude, or what not.

The double aspect of the word as at once a sensory complex and a meaning leads to some difficulty in classification of reactions to it, since a reaction that is associative from the point of view of a word as a sensory stimulus may be an objective reaction from the point of view of the word as a meaning. Thus, the giving of a synonym is probably stressing the objective or pragmatic aspect of a word considered as a meaning, but an associative reaction if thought of as a supplementation of the word taken in itself. Possibly the same statement holds true for the arousal of a simple concrete visual or auditory image of the thing named. In general, the word in the

[3]Myers, *op. cit.*, 13, 1922, 54.

present investigation has been considered a thing-in-itself, as a visual, auditory, and vocal stimulus. Even so understood, however, there are overlappings between the types of reaction to it. For example, it is not always possible to distinguish the character aspect of a word from its objective aspect. This happens, for instance, when there is a distinctive case of word-physiognomy in which the word-form is given by the visual stimulation itself and *not* by the meaning of the word.

I believe my procedure will, in general, be explained by the examples cited under each classificatory head.

The first reaction to every word stimulus was in almost every case and for every *S* some form of inner speech, that is, the word was actually vocalized more or less overtly. Occasionally, the visual glimpse of the word itself conveyed the meaning, but this was not often the case except for *Fs*. The auditory phase of the inner speech varied from *S* to *S*. It was pronounced for *Bu*, *Dj*, and *Fw*. Two *S*s, *Jn* and *Le*, questioned the possession by their inner speech of any auditory quality whatever. *Le* translated auditory content into visual patterns and in her case the auditory aspect was so symbolized. Very little, however, will be said concerning the inner speech phase of the 'word'-reaction; emphasis is placed upon the sequent development of consciousness.

## I. The Objective Reaction. Words as Objects

Comments on the stimulus words as objects occurred frequently in the case of certain *S*s, namely, *An*, *Dj*, *Fs*, and *Fw*. In Myers' reports[4] on individual differences in listening to music the technician often had recourse to such comments. It is evident that a professional interest in any art-material might easily lead to a critical study of it. Only one member of the present group (*Dj*) had such a professional interest in words. Her comments on the words as objects covered a number of phases.

Often the visual word-form was compared with its sound, for example:

> Yew—Charming visually, sound is less so. Word-physiognomy. Word is cone-shaped, like tree.
>
> Fireweed—The sound of the word is less charming than the sight of it.

[4]Myers, *op. cit.*, 13, 1922, 58.

Sometimes a conflict between the meaning and the word-in-itself was evident:

Nightingale—Word in itself not particularly pleasing. Vague visualization of a bird flying low.

Gale—Feeling of swift rush. Heart-palpitation. But *word looks calm.*

Or the typing drew comment:

Dew—Conscious of inequality in type. Unpleasant.

The auditory side of the inner speech is sometimes stressed:

Murmur—Emphasis of auditory aspect of inner speech. Onomatopoetic word. The *sound is* the *meaning.*

Melody—Spoken in light voice (inner speech). Pleasure in dwelling on sound. Onomatopoetic.

*Dj* frequently dwelt on the word-form:

Choral—The word-form is one of roundness. The 'O' is prominent.

Glow—Word looks round and also its meaning. Prolonged 'O' in vocalizing it.

Zither—Fantastic look. The 'Z' looks breezy.

*Bu,* who was musical, often commented on the auditory side of inner speech:

Hiss—Auditory side of inner speech prominent.

Roses—Vocal aspect more prominent than auditory.

Flutter—Auditory emphasis of word itself with repetition to give meaning.

*Dj*'s comments on word-forms have already been cited. It is not always possible, as I stated previously, to draw the line between preoccupation with word-physiognomy and projection into the word of psychic content. This is especially true in the case of *Fs* whose reactions were largely of these two types. Under the discussion of word-personalization, many of her reports will be given. Here a few are included that seem to be primarily concerned with the word-form:

Yew—Vision of hedge fence. Word 'sneers.' Don't like this word.

Jealous—'Snippy.' Looks as if it turned up its nose.

Reed—Word has teeth.

Bower—Looks like a dog. Black woolly dog.

Zither—Zigzag and 'snippy' word.

Word-form, the visual complex of the word as a whole, is, in general, determined by the combination of letters that compose it.[5] Three factors enter into word-form, namely, letter-breadth, letter-height, and the geometrical form of letters. The letters may be classified as those composed of vertical strokes, of curves, of oblique strokes, or of a combination of these strokes. Thus 'm' and 't' are vertical

[5]For a summary of the experimental work on which such a description is based, see E. B. Huey, *The Psychology and Pedagogy of Reading,* 1912, 93 f.

letters; 'a' and 'o' curved letters; 'v' and 'w' oblique letters; 'b' and 'd' a combination of vertical and curved strokes. Now a combination of these various kinds of letters into words determines, in part, word-forms, which give characteristic affective reactions. Words composed mainly of vertical strokes have a 'stiff' appearance, as witness the word 'tiff;' words composed of curved letters are more pleasing to the eye, more rounded and complete in appearance; witness the self-sufficiency of the word 'psychology'! Long letters give individuality to the word-form. Thus 'hypocrite' is a more aggressive word than 'mirror.'

## II. Associative Reactions

(*1*) *Verbal supplementation.* Reacting with any sort of association in *verbal* terms is included under this head.

Verbal supplementation of the stimulus word ranged from the giving of a synonym or an antonym or common extension of the word to quoting a phrase or line in which the word is found. The first three reactions may be called verbal explication; the last, verbal association. Examples follow, the first word is the stimulus term, the rest the response:

Verbal Explication: Drowsy–Sleepy; Kitten–Cat; Sunset–Dawn; South–North; Yule–Yuletide; Rustling–Leaves; Peace–Conference.

Verbal Association: Reapers–Only reapers reaping early; Yesterday–The Road to Yesterday; Reapers–Millet's Gleaners; Cypress–Tree of Lebanon.

Such reactions are commonplace in the literature on association. Their only interest here lies in the great variation in the frequency with which they occurred in the reports of different *S*s when no mental set for verbal association had been established.

*Bu*, for example, reported verbal explication for more than 56% of the words, while *Fs* gave not a single instance of the sort, and *Dj* only 3%.

*Bu* also exceeded all other *S*s in the extent to which extensive verbal associations were given. In over 46% of his reactions such supplementation occurred. Thus, from 'lotus,' he went to 'Nile,' then to 'Egyptian religion,' all verbally. 'Fountain' was completed to 'Fountain of Youth,' than came 'Florida,' then 'Ponce de Leon.' *Fs* gave such verbal associations only twice in 80 reactions (2.5%), and *Dj* only seven times in 100 (7%).

(*2*). *Visual images.* The arousal of images, particularly of visual images, by words is another commonplace of experimental

work on associations. Investigators of imagery frequently rate *Ss* on the basis of the frequency with which visual images are aroused by a list of stimulus words. Other characteristics of visual imagery are probably more important from the esthetic point of view than their frequency of occurrence. As I have discussed this matter at length in another connection,[6] I shall not amplify the topic here. Attention may, however, be called to such attributes of the visual image as its concreteness, particularity, localization, and persistence. The presence of a background for the image or lack of it is also very important and the degree to which the image is under control.

The *Ss* of the present group showed considerable variation in all of the characteristics of visual imagery. Two of them, *Bu* and *Da*, gave so few visual images as to approach a condition of mental blindness. In *Bu*'s case this lack of visual imagery was probably conditioned by extremely poor eyesight. *Fs*, on the other hand, was so preoccupied with visual material that most of her attitudes were dominated by it. Her visual images were more detailed and vivid than those of any other members of the group; she was the only one who could draw expertly. Her interest in visual form was paralleled by her interest in color. *Dj* and *Fw* possessed visual imagery of a diffluent or emotional type. Their visualizations were fluid and evanescent. *Gr* experienced visual images of considerable detail and vividness, but they were wholly without background and very specific.

A few examples of visual images reported by a couple of subjects will be sufficient in the present connection. *Fs* reports as follows:

Lily—Visualization of tall, white lilies in a garden. Then visualization of tiger lilies. A memory of seven years ago. Word makes you feel as though you want to straighten up. Stately word.

Reapers—Visualization of picture in sepia of Millet's Gleaners. The word itself is yellow. Colors seen on the paper; it vanishes quickly. Pleasant word. Energetic feeling. I want to work.

Dews—Visualization of little drops of sparkling water on the ground. Picture of woman with dew-drops falling from her mantle (in Chicago Institute). Smell of dew.

Lily—Lonesome feeling. Visualization of one tall lily in a bare space. Color. The flower is white. A greenish color diffused over everything.

Jasmine—Odor of flower. Visualization of white. Green attitude. Green diffused over everything.

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[6] J. E. Downey, The imaginal reaction to poetry, *Univ. Wyoming, Dept. Psychol., Bulletin No. 2*, 1911, 5-57.

(3) *Auditory images.* Auditory images were not an outstanding feature of the associative reactions. Only one *S* (*Fw*) reported them as a frequent occurrence. In her psychic experiences generally *Fw* is preoccupied with the auditory aspect of things, a preoccupation which I feel sure colors both her everyday attitudes and her esthetic responses. A few examples of *Fw*'s reports follow:

Rustling—Auditory image of soft leaves moving. Attitude of walking in the woods. Visualization of sunlight and shadows.

Jubilee—Awful word. Discordant noises.

Music—Auditory imagery of violin music. Attitude of listening.

Murmur—Auditory imagery of water choked up. Water is dark moss-green color. Afternoon.

(4) *Dated memories, localized or specific images.* The extent to which associations are dated or localized memories or are free from specific temporal or spatial reference is probably of considerable import relative to their value as esthetic material, for in the logic of the emotions with which art is concerned emotional abstracts are of the greatest significance. Dated and localized memories or associations may be too rigidly tied down to permit the freest responsiveness to art. In the present test the range of dated or localized associations ran from 34% for *Le* to 8% for *Dj*.

Although imagery that is too stereotyped or held too rigidly to account in terms of recent or unmodified experience cannot be utilized in the way that old or fluid material can be utilized, one must remember that esthetic appreciation and productivity demand rich psychic content and that such richness can only be gained through availability of past experiences. *La* gave many explicitly dated and localized associations in the word test, but it was evident that in many cases the content might have been shaken free from its particularity and used most effectively. As an example, I may quote a few of her reports:

Gleaming—Visualization of the sun beating down, one day in particular when the alfalfa gave up a blue haze. Whole situation recalled of four years ago. Feeling of pleasure in recalling the situation.

Roses—Visualization of red roses on a bush, in a lane where they were seen frequently up to five years ago. The situation develops. I hear birds, etc.

Dusk—Visualization of our ranch at dusk. General memory. Auditory imagery of cattle and frogs. Lonely but pleasant feeling.

(5) *Literary or art associations.* The frequency with which literary or art associations occurred was of some interest in connection with the problem of the esthetic reaction to words. Tabulation was,

accordingly, made of such reactions. The specific association could, of course, assume many forms. It might be a verbal association such as a repetition of Poe's line "Banners, yellow, glorious, golden" in response to the word 'yellow,' or Swinburne's "I hide my heart in a nest of roses" for 'roses.' It might be a telescoped feeling of Yeats' "Wind among the reeds" for 'reed,' or the visual flash of Burne-Jones' picture in response to 'vampire.'

There was some range of individual variation here which is of interest in connection with the attempt to determine the esthetic interest of the *Ss*. *Da* led with 20%, *Gr* was lowest with 2.5%.

## III. Intra-Subjective Reactions

(*1*) *Organic, olfactory, gustatory, and cutaneous experiences.* The range of difference in the frequency with which individuals reported such material was very great, from 33.7% (*Fs*) to 0% for *Gr*. Such content was practically negligible for *Bu*, *An*, and *Le*. The other 5 *Ss* presented some interesting differences in the kind of sensory material reported. Experiences of coolness and warmth were frequently cited by *Fw* and *Fs*. *Dj* reported very specific organic experiences. Her most frequent reports concerned changes in respiration, such as its retardation or speeding-up. Cardiac sensations were also commented upon, especially palpitation of the heart.

Pendulous—Feeling of being suspended. Breath held. Feeling of wind in nostrils. Word looks high up.

Treasure—Feeling of weight. Expiration of breath. Incipient visual image of earth. Perhaps feel of digging.

Many of these experiences of *Dj* were actual sensations. They are common occurrences in her reactions to poetry, drama, or music, and so pronounced as to make it possible to change the physiological toning of a given day by proper use of art stimulation.

Several *Ss* reported intense organic experiences in the form of drowsiness or giddiness or feelings of heaviness or lightness or localization of sensations in the eyes. These reports bear comparison with those of Bullough's *Ss* in response to color where feelings of weight, or warmth or coolness were reported. One or two specific protocols may be quoted.

*Da:* Quinine—Lump in throat.

*Dj:* Quinine—Bitter. Do I taste 'bitter'? Mouth consciousness with facial reaction to bitter substances.

These reports are of interest for they suggest definite organic conditioning to words. Something similar is found in *Da*'s report for 'gleaming.'

Gleaming—Hurt eyes. Word itself gleaming. Flew up and hit me in the eye.

Two other reports are quoted to show how these physiological reactions give intimacy and richness to the psychic content.

*Dj:* Primrose—Rich associational consciousness. Flicker of yellow and of yellow flowers. Sense of pollen on hand and in nostrils.

*Fw:* Wind—Auditory image of a big roaring sound. Dusty grey sound. Time, quite late in the afternoon. Feeling of dust in the eyes.

(*2*) *Attitudes and activity tendencies.* By attitudes, I refer to unanalyzed experiences, set usually in a motor framework. Besides attitudes, there were all kinds of incipient and actual movements. Such reactions were common except for *Gr* and *La*. The following reports are typical:

*An:* Rhythm—Kinesthetic consciousness of running hand up and down. Tapping measure.

Laughing—Kinesthetic consciousness of lips and mouth as though laughing. Strain in sides of face.

Welcome—Feeling of meeting someone and being cordially received. Kinesthetic.

*Fw:* Blue—Monday attitude. Hate-to-get-out-of-bed feeling.

Zigzag—Feel of zigzagging.

Melody—Ample sound. Violin. Evening. Feeling of crowds of people around.

*Bu:* Hiss—Mouth attitude of disapproval.

Zigzag—Feeling of going back and forth. Meaning expressed by attitude. Feeling of syllabification.

Eye-consciousness is reported on occasion by a number of *S*s. This eye-consciousness may be organic as described previously or it may involve focusing or relaxation of the eyes or other ocular movements. The following examples are taken from the protocols of *Dj*:

Clapping—Said word with, perhaps, a sort of flicker of the eyelids.

Shadow—Mood-tone. I think my eyes moved toward the right.

Sunset—Feeling of turning west and of the eyes taking in the glory of sunset.

(*3*) *Affective and emotional experiences.* Organic and attitudinal reactions shade into affective and emotional ones. Obviously classification is at times arbitrary. At others, the pattern is distinctly that of an emotion or mood.

*Gr* and *Le* gave few of such reactions; *Da*, *La*, and *Dj* many of them.

*Da:* Wings—Just a mood. Happy.

*Da:* Rustling—Hear leaves rustling. Emotional word. A reference to self. Pleasant. Dreamy. Lying under trees, listening.

*La:* Melancholy—Spoke word aloud. Visualization of a dark rainy day. Auditory image of rain. Feels self inside a house looking out. Feeling of melancholy.

*Fw:* Cypress—Funeral feeling. Said 'funeral.' Rain dropping. Auditory and cutaneous imagery of rain. Visual image of people around grave.

(*4*) *Specific self-references.* Specific self-references were not common except for *Da*, one of the most emotional of the group.

Dew—I myself out in field, very early in the morning, attitude of standing.
Star—Felt self out at night, looking up at sky. Feel feet on the ground.
Wreath—Felt someone putting wreath on my head.

Self-projection *into* an imaginal situation is the usual reaction for *Da* in contrast to *Le* and *Gr* who sometimes have recourse to self-visualizations but keep on the margin of imagined situations.[7]

## IV. Symbolization

Three types of symbolization will be illustrated: (*1*) word-coloration; (*2*) visual patterning of auditory content; (*3*) time-of-day connotation. Myers describes such symbolization as "a half-way elaboration of the object-matter, not reaching to the height and the free organic independence of the character aspect, but kept low by the trammels of associative and intrasubjective factors."[8]

(*1*) *Word-coloration.* Word-coloration is a form of synaesthesis with which we are familiar from reports on such experiences. It is, probably, fairly common in the form of pseudosynaesthesis, in which the color serves as a sort of emotional symbol, but relatively infrequent in the sense of a genuine secondary sensation.

Three *S*s of the group under consideration reported word-coloration to a considerable degree; *Fs* for 58.7% of the words reacted to; *Fw* for 28.8%; *Dj* for 13%. Other *S*s reported occasional instances of it.

The reports of *Fs* suggest a case of true synaesthesis, although the degree to which the color was projected varied from one word to another. Frequently the paper on which the word was typed appeared colored in a small area surrounding the word; at other times, the word appeared written in color. On certain occasions the color

[7]For a discussion of the esthetic problems of self-projection, see J. E. Downey, Literary self-projection, *Psychol. Rev.*, 19, 1912, 299-311.

[8]Myers, *op. cit.*, 13, 1922, 66.

was localized on the eyes and in a few instances reported as merely the feel of a color. This *S* has color moods. For example, happy days are pink days. At such times, everything felt "pink," and might be suffused with this color. Aesthetic moods were also rose-colored. The day of the test was a "pink" day. A few specific reports of her color reactions follow:

Bells—Colors run downward on the eyes. All sorts of colors.
Golden—The word appeared written in gold.
Heroic—Brown on paper. Feeling of desire to laugh. Noble look (to word). "Oh, I am the whole thing!"
Jasmine—Pinkish-purple color. Localized on eyes.
Jealous—Word appeared written in ugly green.
Laughing—Very delightful red on the eyes. The red persists.
Melancholy—Green and purple word. Color projected on paper.
Music—Yellow and pink shading into each other on the eyes.
Nightingale—Flash of bluish-violet on paper.
Nocturne—Green was localized on the eyes and on the paper.
Wind—Kinesthetic consciousness of sway of wind and streak of it through the trees. Cutaneous feel of wind blowing. One end of the word is green on the paper.

*Fw* gave color reactions for more than 28% of the words reacted to. Her color reactions were much more evidently emotional and subjective than were those reported by *Fs*. Frequently they represent an abstraction of the color from a scene or object thought of or they are symbolizations of feelings. The following reactions illustrate these statements:

Bells—Colored sounds. Silver.
Buzz—An auditory image of a sawmill is heard as gray. The noise is colored gray.
Dawn—Visualization of gray and pink streaks.
Drowsy—Visualization of a golden haze. Reaction projected into the word.
Dusk—Soft grays, greens and violets, melting one into the other.
Heroic—Latin-lesson feeling. Caesar's bridge. Gray haze.
Jasmine—Green diffused over everything.
Kitten—A kitten is heard purring. The purr is pale blue.
Music—Purple-violet haze over everything.
Primrose—Pink color projected outside.
Shadow—A mental darkening. Then the room actually darkens.
Thunder—Auditory image of far-off roaring thunder. The sound is dark.
Turrets—Very dark green color with black streaks.
Twilight—All sorts of dark colors, as purples, grays and greens.
Wind—An auditory image of a big roaring sound which was dusky gray.

*Dj* has had sporadic experiences of true synaesthesis in connection with shrill sounds and occasionally with music. Pseudochromaes-

thesis is common in connection with music, voices, and proper names. Thirteen of the words reacted to in the present experiment gave color experiences ranging from objective projected colors to subjective colors. Examples are given below:

Blue—The word looks blue. Cannot identify tint. Navy? Azure?
Bower—Word looks round and a trifle illuminated.
Gleaming—Eye-consciousness such as would accompany actual gleaming. Some light to word as seen.
Lily—Word looks white.
Thunder—The word looks heavy and dark.
Twilight—First three letters look dark. The word as a whole appears shadowy.
Vermillion—The word looks red.

(*2*) *Visual patterning of auditory content.* This form of symbolization occurred primarily for *Le* who frequently translates auditory experiences into visual patterns. Her description of her customary translation of voices is most interesting. She writes:

When listening to a lecturer, I sometimes become so interested in the color and geometric form of the sound of his voice that I fail to get any meaning from his words. I remember a recent speaker's voice. It was a polished white, was octagonal in shape, and was almost as large as the platform. It seemed to be just in front of him. If I do not watch myself, I always get this form rather than content. Nearly every voice is colored, never brilliantly, and all have some geometric form, never curved. Each such figure is definitely located in space, according to its shape and size. Spoken words, for me, are printed in space in front of me, in large or small type, according to the quality of the voice. The lecturer, of whom I have spoken, had his words printed in small type, and quite a distance above his head."

*Le* gave a few instances of visual translation for auditory reactions in the present test, among them the following:

Clapping—Visualization of roomful of children with optical-kinesthetic imagery of clapping movement. Auditory content is given by visualization *at level with eyes.* Inner speech also has position, low in the throat, but not projected.
Drowsy—Auditory (?) consciousness of buzzing bees. Low level, like corkscrew.

(*3*) *Time-of-day connotation.* One curious symbolization reported by *Fw*, for nearly one-fourth of the words reacted to, was the feeling of a particular time of day. This time-connotation was quite distinct from the report of memory images that have a definite temporal setting. *Da*, who gave a considerable number of dated associations, sometimes abstracted the temporal aspect of a situation and generalized it in such a way as to suggest that as a further develop-

ment she might have reported some such experience as *Fw*. On the whole, however, *Fw*'s reports were in this respect highly individualistic. Examples follow:

Bells—Night feeling.
Drowsy—Saturday afternoon word.
Ghost—About dawn.
Kitten—Noon-time.
Lotus—Late afternoon feeling.
Melody—A night-word.
Murmur—Afternoon.
Peace—Night.
Roses—Late in the day.
Vagabond—Early in the morning.
Wind—Quite late in the afternoon.
Woodland—Early in the morning.
Zigzag—Night-time.
Zither—About ten o'clock in the morning.

## V. Personalization

(*1*) *The character personified as subject.* By personalization is meant the projection of psychic content into a word. The psychic content may be of any sort, sensory, conative, or affective. Such projection is usually thought of as preëminently the esthetic reaction. It signifies an objectifying of subjective experience, the detachment of it from personal anchorage, the giving it "distance" (in Bullough's terms).

In the word-test, 4 *Ss*, *Dj*, *Fs*, *Fw*, and *Da*, reported a very considerable amount of word personalization. *Da* and *Fw* made, explicitly, distinctions that are quite in harmony with current esthetic theory, although neither of these *Ss* was at all conversant with esthetic theory.

*Da* distinguished between what she called atmosphere words and emotional words. She explained the difference in this way: "Emotional words give me a mood or feeling, and in atmosphere words this feeling belongs to the words themselves." "Melancholy," she called an emotional word, but "mystic" was an atmosphere word bringing with it the feeling of Tennyson's King Arthur Cycle, particularly the feeling of the Holy Grail.

*Fw* also made a clear-cut distinction between words into which the reaction was projected and those in which it was not.

Sunset—It is an emotional word. Gives a stormy feeling but the feeling not projected into word as occurs for 'dawn' and 'shadow.'

*Fw* listed such words as 'dawn,' 'drowsy,' 'shadow,' and 'dusk' as emotional words in which the reaction was projected into the word itself. A number of her protocols follow:

Dawn—An emotional word. Visualization of gray with pink streaks. Beautiful word. Feeling of stillness and beauty. Latent auditory image of morning sounds. Self placed on a road facing east. All this in word. The feeling is projected.

Jewel—Emotional word with content projected into word. Dark red haze. Feeling of something wrong. Placed inside the earth. An after night word. Uneasy feeling.

Twilight—Emotional word. Emotion projected into word. All sorts of dark colors—purples, grays and greens. Feeling of a particular wood on a certain road. Whip-poor-will singing.

*Df* frequently projected emotional reactions into words. Such words she labeled poetic words or words with a mood-tone. She also projected the organic resonance into words. The following are examples:

Twilight—A mood-word, looks its meaning. Lovely sense of relaxation and of dim light (pleasant and reposeful). First three letters look dark. Seems to be some sense of odor. A cool, delightful fragrance. Word looks shadowy.

Woodland—A mood-word. Rich complex of incipient images. Small trees and birds and flowers and brooks. Scents and sounds. Word looks woodsy.

Thunder—Voice deepens in speaking it. Smell of rain. Incipient auditory image. Word looks heavy and dark.

*Fw* projects into words all manner of psychic content, sensory, emotional and attitudinal:

Yesterday—Yellow, actually saw yellow on the paper. A laughing word. Word grins. Could hold this word in my hand, could pick it up and hold it. Happiness, general mood.

Buzz—Vision of a small boy. Swarm of bees. Rough word. Rough to finger. Black and yellow on eyes.

Twilight—Gleaming color. Sparkly lights from house. Feeling of sleepiness. Word looks half asleep. Blinky.

Lotus—Word written in white on the eyelids. Tastes and looks sweet.

Zigzag—Light in zigzag (visualization). Laughing word. Happy-go-lucky. Tatters on word. Ragged. Very disreputable, but awfully nice.

*Fs*' comments on 'zigzag' illustrate a reaction that was very common with her, a preoccupation that is to say with word-physiognomy. Apparently in her case the visual word-form itself was extremely potent, more so than with any of the other *S*s. Whereas, for most *S*s the word-form was swamped by the meaning, for *Fs* the pattern released by visual stimulation was the potent thing. Her reaction to

the word 'murmur' suggested a curious translation of the auditory onomatopoeia commonly associated with this word:

Murmur—Green color on paper. Brook. Can feel ripple in word. You can pick this word up and stroke it.

## VI. The Total Response

(*1*) *Incipient imagery. Latent content. Conflicts.* In citation of the word reactions the total response has at times been torn apart in order to permit classification of its several aspects. Such fragmentary presentation does violence to the total reaction which may be very rich. Several examples may be cited giving the total response:

*An:* Nature—Lots of different feelings. Kinesthetic consciousness of looking at a wood with many green things. Visualization of a great expanse of barren country. Kinesthetic feeling of being very small. Kinesthetic consciousness of turning head and looking in all directions. Shrinking of self and widening out of everything else. Feeling of being five or six inches tall.

*Bu:* Fireweed—Said word aloud. Feeling of not getting the meaning. Unrest. Tightening of muscles. Verbal association 'weed.'

*Fs:* Lotus—Vision of self picking blossoms and eating honey. Memory eleven years old. Sees self as then but now wears sunbonnet such as was never possessed. Word written in white on eyelids. Tastes and looks sweet.

Rustling—Lovely word. Leaves falling. Gorgeous colors. Whole scene in detail, partial memory of years ago. Children playing on a hill. I am one of them. Word crackles. Is a green word in head. Get feeling of green leaves as though drawing fingers over leaves.

*Dj:* Sunset—Mood-tone immediately on glancing at word. Visual aspect of word significant. Feeling of turning west and taking in glory of sunset. Also restful feeling. Suspended breathing. Very poetic. Repeated word several times. Word-physiognomy. Enclosure complete.

## Conclusions

The main value of the material that has been presented in the preceding pages is, as was stated previously, the comparison it makes possible of word-reactions with those to tones and colors. The responses to these three different forms of stimuli are found to be closely comparable.

Individual differences have also been stressed because of their supposed connection with differences in esthetic responsiveness. This topic demands amplification. Of the *Ss* of the present investigation, *Dj* and *Fw* possessed the greatest interest in literature, particularly poetry. *Dj* has published both poetry and plays. *La* was interested

TABLE I

PERCENTAGE OF DIFFERENT TYPES OF REACTIONS TO STIMULUS WORDS. All Subjects

| Types of Reaction | *Ss* | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | *An* | *Bu* | *Da* | *Dj* | *Fs* | *Fw* | *Gr* | *La* | *Le* |
| Critical comment | 17.5 | 11.2 | 7.5 | 25 | 28.7 | 12.5 | 0 | 0 | 0 |
| *Associative* | | | | | | | | | |
| Verbal explication or supplementation | 37.5 | 99.0 | 25.0 | 10 | 2.5 | 14.0 | 7.5 | 15.8 | 20.0 |
| Visual imagery | 55.0 | 6.2 | 17.5 | 29 | 100.0 | 36.2 | 61.2 | 72.4 | 74.0 |
| Auditory imagery | 3.7 | 3.7 | 15.0 | 3 | 8.8 | 37.5 | 1.2 | 18.8 | ( 8.3?) |
| Dated and localized associations | 16.2 | 12.2 | 20.0 | 8 | 23.7 | 10.0 | 10.1 | 24.6 | 34.0 |
| Art and literary associations | 12.5 | 17.5 | 20.0 | 15 | 7.5 | 8.7 | 2.5 | 17.4 | 14.0 |
| *Intra-Subjective* | | | | | | | | | |
| Physiological reaction | 3.7 | 1.2 | 17.5 | 29 | 33.7 | 25.0 | 0.0 | 26.1 | 3.3 |
| Attitudes and kinesthetic reaction | 42.5 | 21.2 | 40.0 | 31 | 30.0 | 35.0 | 6.2 | 13.0 | 18.3 |
| Affective and emotional reaction | 16.2 | 12.5 | 25.0 | 26 | 17.5 | 15.0 | 1.2 | 37.7 | 1.6 |
| Self-reference | 13.7 | 1.2 | 23.7 | 4 | 11.2 | 6.2 | 2.5 | 5.8 | 4.0 |
| Symbolization | 8.7 | 0 | 3.7 | 14 | 58.7 | 28.8 | 6.2 | 0.0 | 11.6 |
| Personalization | 7.5 | 2.5 | 13.7 | 47 | 43.7 | 16.2 | 0.0 | 1.4 | 0.0 |

n short-story writing. *Bu* was an unusually fluent debater and musically gifted, possessing a tenor voice of beautiful tone-quality; since his college days, he has become a very successful lawyer. *An* was interested in debating and in journalism; he is now a well-known psychologist. *Fs* was the most gifted of the *S*s in the visual arts; she drew well and did clever things in the making of jewelry. She was not in the least gifted in literary expression, partly because words were such isolated things or objects for her that she found it difficult to bring them into fluid relationships in sentences. Her major interests in college were mathematics and chemistry. She is now a teacher of mathematics. *Da* was highly appreciative of art but had done no art work. *Le* and *Gr* were somewhat matter-of-fact individuals and unimaginative. *Le* majored in college in mathematics, in which subject she did excellent work.

A glance at the summary in Table I is most instructive when taken in connection with the above descriptions. *Fs*, *Dj*, and *An* gave the greatest amount of critical comments. For *Dj* and *An*, this may represent a somewhat technical interest in words. The same statement may hold true for *Fs* although her interest in words was largely limited to the visual aspect and would find a technical application only in some forms of advertising. Her comments on words are paralleled by those of the poet Gautier who writes, "For the poet, words have in themselves and apart from their meaning a beauty and value of their own as gems which have not yet been cut and mounted in necklaces, bracelets, rings."[9]

*Bu* excelled in the amount of verbal explication and supplementation. He might in fact be spoken of as dictionary-minded. His interest in discrimination of meanings has probably done him excellent service in his chosen profession of law.

*Fs* also showed a great interest in concrete visual material, as evidenced by the large number of visual images aroused in reaction to words. *Gr*, *La* and *Le* were also very visual in their reactions. Auditory imagery was important only for *Fw* and *Da*; for them it definitely enriched their responses. Localized and dated associations were particularly evident in the reports of *La*, *Le* and *Fs*.

For the total frequency of intra-subjective reactions the order, from least to greatest, was as follows: *Gr*, *Le*, *Bu*, *Fs*, *An*, *Fw*, *La*, *Dj*,

[9]Quoted by A. Antheaume and G. Dromard, *Poésie et folie*, 1908.

*Da.* On the whole, the *S*s most responsive to esthetic appreciations were those giving the greatest number of intra-subjective reports.

Symbolization was a reaction that was common only for *Fs*, *Fw*, *Dj*, and *Le*. Myers, as we have seen, considers this reaction a halfway one, one that has not completely achieved the detachment from subjective interests but is on its way to becoming esthetic. Personalization of words occurred frequently only for *Dj*, *Fs*, *Fw*, and *Da*. It is usually recognized as the esthetic response; writers on esthetics in general describe esthetic responsiveness in terms of impersonality, disinterestedness, distance.

In order to make clearer the nature of the individual differences in reaction, the reports of the 9 *S*s on two different words are given. A gradation is attempted from least to most poetic reaction. The first word is 'cypress:'

*Gr:* Auditory vocalization of word. Nothing else.

*Le:* Visual image of tree. (Cypress?)

*La:* Spoke word aloud. Visual image of cypress tree with low hanging branches. Feeling of coolness.

*Bu:* Said 'Tree of Lebanon.' Bible feeling.

*Jn:* Spoke word aloud. Visualization of tree. Memory of a picture. Vines hanging to tree. Feeling of a verse of poetry.

*Da:* Spoke word aloud. Said 'cypress tree.' Definite literary atmosphere to word.

*Fs:* Said word. A dark word. Visualization of tree in a dark garden. Almost dark. A storm coming on. No grass. Thick shade. Earth wet and damp. Dignified word and rather mysterious.

*Fw:* Funeral feeling. Said the word 'funeral.' Auditory and cutaneous image of dripping rain. Visual image of circle of people around a grave. Feeling of an incipient picture which I can't recall.

*Dj:* Tree-mood. Visualization of a dark, tall, isolated cypress. Feeling of loneliness and of shadows. Word looks remote, chilly, shut-in.

For 'drowsy:'

*Gr:* Said the word over and over. No further reaction.

*Jn:* Kinesthetic feeling of throwing the head forward. The word breaks between the 'w' and 's.'

*Bu:* Attitude of being surrounded by foliage. Verbalized the synonym 'sleepy.' Recollection of an attitude of drowsiness.

*Da:* Mood of feeling sleepy at once. I feel myself lying down at home, then in the country.

*Le:* Visualization of summer meadow with stream and trees and flowers. Memory of childhood home. Auditory (?) image of buzzing bees, seen at low level, like a cork-screw. Drowsy feeling from lazy drowsy summer time and buzzing of bees.

*La:* Sensation of being drowsy. Eyes feel heavy. Word said over and over slowly as though uttered by hypnotist.

*Fs:* Beautiful rose-color. Sleepy. Visualization of yawn around word. Word is soft. Like to pat it. I like this word, Visualization of fireplace with little child.

*Dj:* Organic consciousness at sight of word. Heavy eyelids and falling respiration. Word looks its meaning. Word spoken with prolonged inflection. Organic mood-toning. Poetic word.

*Fw:* Emotional word. Said the word aloud. It says itself yawningly. Attitude of summer afternoon on back porch. Lazy afternoon sound of bees humming. Visualization of color. Golden haze over everything. Reaction projected into word.

Bullough believes that the character-aspect is derived from the physiological, a statement which Myers is not ready to accept, nor does the latter report 'purity of type' as frequently as does the former. There is some evidence in the word test that *Ss* who report much personalization of material also report much intra-subjective material, although the converse does not always hold true. There is little evidence of 'purity of type' in reaction to words. The total response is often a complex one of great variety. The projection of psychic content into words is abundantly attested, but its significance should be more completely analyzed.

Richards' comment on impersonality in art deserves careful consideration:

> To respond, not through one narrow channel of interest, but simultaneously and coherently through many, is to be *disinterested* in the only sense of the word which concerns us here. A state of mind which is not disinterested is one which sees things only from one standpoint or under one aspect. At the same time since more of our personality is engaged the independence and individuality of other things becomes greater. We seem to see 'all round' them, to see them as they really are; we see them apart from any one particular interest which they may have for us. Of course without some interest we should not see them at all, but the less any one particular interest is indispensable, the more *detached* our attitude becomes. And to say that we are *impersonal* is merely a curious way of saying that our personality is more *completely* involved.[10]

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[10] I. A. Richards, *Principles of Literary Criticism*, 1926, 251 f.

# APPARENT VISUAL MOVEMENT WITH A STATIONARY STIMULUS

By Herbert Sidney Langfeld, Princeton University

The phenomena of apparent visual movement have been studied intensely by a number of psychologists during recent years, and their results are too well known to need description. Most of the apparent movement which has been investigated has been obtained by the stimulation of neighboring points of the same retina by two or more stationary stimuli which were exposed successively. The experiments which will be described in this paper were undertaken primarily for the purpose of studying the nature of the perceptions produced by the stimulation of the two eyes successively by the same stationary stimulus. In other words, successively right and left monocular vision was employed to observe the same stationary object. In the course of the experiments changes were introduced in the form of stimulation in order to analyze the experience more thoroughly. Some of the results were not produced by a single stimulus, and others do not bear directly upon the problem of apparent movement. All of the results, however, which pertain to visual perception will be included in this report.

In the experiments here reported, I was myself, for the most part, both experimenter and observer. Repeated observations were made under each condition during a period of about four months. In order to check up the results, the following *S*s were used in each of the experiments: Drs. E. G. Wever and W. S. Hulin, Messrs. H. Schlosberg and C. W. Bray. Professors E. B. Holt and L. Carmichael also observed under most of the conditions. Unless otherwise stated, the results which are here described were corroborated in every detail by the *S*s who observed under the conditions in question. The experiments, with the exception of one series, were conducted in a well-lighted room. A single series was performed in a dark room.

*Apparatus.* Three different forms of apparatus were used. Fig. 1 is an illustration of the perpendicular exposure apparatus. The front upright board is approximately 50 cm. x 25 cm. There are two round holes, 2½ cm. in diam., in this board, the upper edge of the holes being 5 cm. from the top of the board. The holes are 6 cm. apart from center to center. In front of the holes is fastened

the ocular end of an ordinary stereoscope. The white disk is 49 cm. in diam., and revolves on an axis which is located on a line running horizontally through the center of the two round openings in the board. The axis is 15½ cm. from the middle point between the two openings. Two square holes, with sides of 2½ cm., were cut in the disk and were placed 2 cm. apart. With this distance there was a slight overlap of the exposures of the two eyes. That is to say, there was a short period of simultaneous exposure to the two eyes of a part of the stimulus field. To make the exposures, the disk was moved back and forth through a small sector just large enough to expose first one eye and then the other. With this disk, the exposure is from the upper to the lower point of the round holes and vice versa. This apparatus will be referred to as the *P.E.* apparatus (perpendicular exposure).

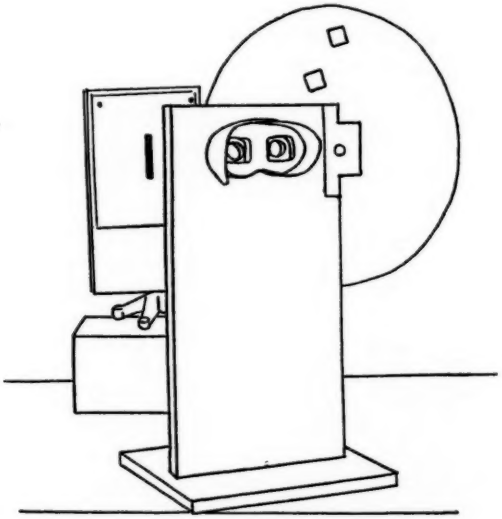

FIG. 1

The second apparatus is identical with the first except for the disk. In this case, the axis of the disk is placed below the eye-piece on a perpendicular line passing through the mid-point between the two round holes and 13 cm. below this point. Only one square hole is necessary with this arrangement, and is placed so as to expose the stimulus field from *r* (right) to *l* (left), and vice versa when the disk is revolved. With this apparatus, exposure to one eye is complete before the exposure to the other eye begins. The term *H.E.* apparatus (horizontal exposure) will be used for this arrangement.

In the third apparatus two camera iris shutters were used. These shutters are fastened into two oblong pieces of bakelite which move horizontally in grooves on a board with an opening behind the shutters. The board is clamped to two

standards, and can be moved up and down for adjustment to the height of the subject's eyes. The shutters can be adjusted horizontally to correspond to the interocular distance of the eyes. The shutters were set at 'bulb,' and were operated by the subject. The term *I.E.* apparatus (iris exposure) will be applied to this apparatus.[1]

*Experiment 1.* The *H.E.* apparatus was used. The stimulus was a perpendicular black stripe 1 cm. x 8 cm. on a white background. The stripe was at the level of the two eyes, and usually about 50 cm. from the eye-piece. The disk was moved by the *S* in all of the experiments.[2] Each eye was exposed alternately a number of times and with varying speed.

With very slow exposure the stripe was seen displaced to the *r* by the *l* eye, and to the *l* by the *r* eye (it was only by closing and opening the eye that one knew which eye was seeing the stripe). With increase of speed, the stripe seemed to move out from behind the shutter as the latter moved across the field of vision exposing the stripe. The *r* eye saw the stripe move into view from *r* to *l*. The shutter then covered the *r* field and exposed the *l* field. The stripe was then seen by the *l* eye to move out from *l* to *r*. There was a distinct perception of the stripe moving out, one might almost say with a jump, from behind the shutter from the *r*, then from the *l*. With increased speed of exposure, the stripe moved from *r* to *l* and from *l* to *r* across the white background, and was seen continuously as a single stripe. With still greater speed, the single line appeared continuously in the center of vision with no movement, or at most slight oscillations. I found that after a long period of experimentation at very rapid speed, the single image tended to break up into double images, due undoubtedly to muscular fatigue and change of convergence. The perception was then of two stripes with no movement.

Several variations were made. The stripe was exposed for some seconds to one eye, and then quickly exposed to the other eye. Good movement was obtained. The stripe was also placed horizontally on the background. It then moved horizontally back and forth somewhat like a battering-ram. At times the line, both when in horizontal

[1]The apparatus was made up in these simple forms for the preliminary work. The *H. E.* apparatus was used first, and the changes were suggested during the progress of the experiment. It is planned to make an apparatus which will permit variations in size of exposure opening, space between exposures, duration of exposures, etc.

[2]I found that I could analyze the situation better if I regulated the rate and number of exposures myself, and that I could get clearer results from other *S*s if I allowed them to do the same.

and in vertical positions, seemed to move diagonally from lower *l* to upper *r*, and the reverse. This change of direction was apparently due to a change of position of the eyes.

*Experiment 2.* In order to determine what effect, if any, the movement of the shutter had on the perception of apparent movement, the *P.E.* apparatus was used with the black stripe on a white background as stimulus. It will be remembered that with this instrument exposure of one eye begins before the exposure of the other is finished. Full movement was even more easily obtained than with the *H.E.* apparatus. End movements from *r* to *l* and the reverse appeared with slow speed of exposure, but there was no effect of the stripe moving out from behind anything. With very slow speed, however, there was at times a perception of the shutter taking the stripe off the background with an upward movement when it appeared at the *r*, and with a downward movement when it appeared at the *l*. It was somewhat as if the shutter erased the stripe at the *r* with an upward stroke, and on the *l* with a downward stroke. There was then no perception of horizontal movement of the stripe, although there was a perception which was as if something had moved horizontally. The vertical movement of the stripe very readily went over into horizontal movement of the stripe. The nature of this change was somewhat like that of reversible perspective. With very fast movement there was the perception of a single stationary stripe which at times seemed to me to have a slight movement up and down.

*Experiment 3.* The *I.E.* apparatus was devised to eliminate the influence of both the horizontal and vertical movement of the shutter. The black stripe on the white background was used as the stimulus. Movements were obtained similar to those in the previous experiments. There was, however, no perception of the stripe being moved, or of jumping out from behind anything.

*Experiment 4.* The *H.E.* instrument was used. Black stripe with white background. The disk was started with the exposure window between the two eyes, neither eye being exposed. The disk was moved toward the *r* exposing fully the *l* eye, and then to the *l* just far enough to expose the stripe to the *r* eye, but none of the left side of the background. The shutter was held stationary for about ½ sec. while the stripe was exposed to the *l* eye before closing that side. With a little practice, this form of exposure could be made in rapid succession. The *l* eye always saw the total field, the *r* eye only the field to the right of the stripe and the stripe itself. The following

phenomenon was perceived by all of the *S*s who performed this experiment. The stripe appeared on the fully exposed side, and disappeared with no movement. The stripe then appeared on the half-exposed side, usually with no movement, and with the closing of the shutter seemed to be pushed in a downward curve across the half of the field until it disappeared. It was as if the shutter pushed the stripe before it. Movement was perceived over the entire half of the field, although toward the end it sometimes seemed more in the nature of 'pure' movement. The perception of the movement of the stripe was so vivid that one of the *S*s did not realize the stripe was actually exposed only at the beginning of the movement, during the interval that the shutter passed over the 1 cm. width of the stripe. It should be emphasized that the direction of this apparent movement is the reverse of that movement which would have occurred if both eyes had been fully exposed.

A variation of the perception was to see the stripe snap into position again with a slight end movement, as if it had jumped from behind the shutter. This same *S* found that "if the shutter was not in attention, the line seemed to move of itself." Only when the shutter became part of his perception did it seem to push the line away.

The exposure was then changed in the following manner: the shutter was moved back and forth in such a way that it only exposed the stripe and half of the field to each eye. The stripe appeared without movement in the center of the field, and was then pushed in a downward curve toward the *r* and disappeared. It appeared again in the center of the field, and was pushed in the same manner toward the *l*. The perception was that of a stripe being pushed off from the center first toward one side and then toward the other. Again the direction of movement was opposite to that which would have occurred if there had been horizontal movement under the condition of exposure of the entire field to each eye.

It is evident that in this experiment the movement of the shutter entered into the total perception as a strong determining factor.

*Experiment 5.* Instead of the straight black stripe, a semicircular black stripe, 4 mm. broad with a diameter of 9.5 cm., was used. The semicircle was placed in a perpendicular position. Experiments were performed on all three types of apparatus. Various phenomena were observed. Usually the semicircle moved back and forth as a whole. Sometimes the center moved in and out, and the

ends were stationary. At other times, the center remained stationary and the ends moved back and forth. One *S* found that frequently the bottom of the curve did not move, but the top "tended to turn." By voluntarily fixating the center of the perpendicular diameter, this *S* found that the semicircle moved as a whole. The difference in movement seemed to be caused by difference in fixation. Several *S*s found that by changing fixation they could get different parts of the curve to move, while the rest of the curve remained stationary.

*Experiment 6.* The *I.E.* apparatus was used. Two parallel and perpendicular black stripes with approximately interocular separation. The stimulus was moved toward the eye until the *r* eye saw only the *r*-hand stripe, and the *l* eye the *l* stripe. With a long interval between exposure of the two eyes, first the one stripe and then the other stripe was seen. With rapid succession of exposure both

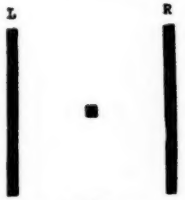


FIG. 2.

stripes were seen simultaneously. Most *S*s got no movement at any of the speeds. I seemed to detect slight end movements at times. It may be that with a smaller distance between the two stripes movement could be obtained, but with this set-up it was not possible to bring the stripes much closer together without both of the stripes being seen by each eye, in which case both lines would move together horizontally as a unit.

*Experiment 7.* With the *I.E.* apparatus. Two black parallel stripes with a small square dot half-way between them (Fig. 2). The stripes were movable, and were separated to approximately the interocular distance. The diaphragms of the shutters were closed sufficiently to shut out the *r* stripe from the *l* eye and the *l* stripe from the *r* eye. The *r* eye, therefore, saw the *r* stripe and the dot; the *l* eye saw the *l* stripe and the dot. The two shutters were then opened and closed alternately, thus exposing the stimulus to the two eyes successively. The usual experience with a medium speed of alternation was to perceive simultaneously the two stripes which re-

mained stationary, while the dot moved back and forth between them with a clear, full movement. At times it seemed to me as if one of the stripes moved with the dot, while the other stripe remained stationary. The outstanding result of the experiment was the fact that all of the subjects got the phenomenon of two stationary stripes and the moving dot, which was clearly perceived as a single dot throughout the full extent of the movement. With slow speed, the stripes appeared alternately without movement, and there was an end movement of the dot.

*Experiment 8.* With the *I.E.* apparatus. Same stimulus as in *Experiment 7.* This experiment was performed in the same manner as the previous one, except that before starting the successive exposures the subject obtained a double image of the dot, and change in location of the stripes by change of convergence. The subject

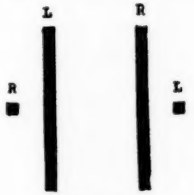


FIG. 3

converged sufficiently far beyond the plane of the stimulus to obtain a perception as in Fig. 3. The stripes came in toward the center, crossing the double image of the dot. The *l* eye saw stripe *l* and dot *l*, the *r* eye stripe *r* and dot *r*. The perception with successive exposure was that of a single line moving horizontally back and forth, and a single dot moving in the opposite direction across the stripe. When the stripe moved to the *r* the dot moved across it to the *l* and vice versa. Sometimes it seemed to me that the dot jumped across the stripe, or swung out from the background and across the stripe in a curve. In this latter case, it seemed as if the stripe were a pole and the dot a ball attached to the end of the pole by a string. When the pole moved to the *r* it swung the ball around in a circle toward the *l*, then swung it back again to the *r* as the pole moved to the *l*.

I was able once or twice to converge sufficiently behind the stimulus to bring the stripes on the fovea of each eye, thus causing fusion, and to hold the eyes in this position during successive exposures. The perception with both eyes exposed was a single stripe

between two dots, as in Fig. 4. With successive exposure, the perception was that of a single stationary stripe. The dots were seen as a single dot moving back and forth across the stripe. It was very difficult to hold this exact convergence. The stripe would soon break into double images, and be perceived as a single moving stripe.

*Experiment 9.* With the *H.E.* apparatus. Black stripe on white background. The stimulus was so arranged that the *r* eye saw the stripe on a white background. The *l* eye saw only the white background and not the stripe. With exposure of the background with stripe, and then just background, the stripe appeared and then disappeared with an end movement. Two different perceptions could be observed. (1) The shutter was placed so that the *l* eye saw the white background. The *r* eye was closed. The shutter was then moved to the *r* exposing the stripe to the *r* eye. The stripe seemed to jump out on the white background toward the *l* from behind the shutter. The shutter was then moved toward the *l*, closing the *r* eye and exposing the white background to the *l* eye. The stripe seemed

FIG. 4

to move across the background in 'pure' movement. In this perception, the apparent movement was in the opposite direction from the movement of the shutter. (2) The shutter was moved the same way as previously. The stripe seemed to be dragged behind the shutter and to be left on the white background. It continued to move slightly in the direction of the moving shutter after it was actually seen. Before it appeared there was only a feeling that it was being moved behind the shutter. When the shutter came back toward the *l*, it pushed the stripe slightly toward the *l*, then covered it and seemed to drag it out of the field of vision. Again one only 'felt' that it was being moved away behind the shutter. We have here again a reversible perception. So far as could be observed, the outward conditions had not changed, but the direction of the apparent movement was reversed in the two experiences.

In order to test whether it is necessary to have the exposure of the background or some other stimulus to one eye alternately with exposure of the stripe to the other eye, the stripe was exposed repeatedly at short intervals to one eye, the other eye being kept closed. No movement was observed. The movement of the shutter cannot, therefore, be considered the primary cause of the movement. Experiments were also made with the *I.E.* apparatus, the stripe being exposed to one eye, the white background to the other. Good end movements were observed, but the effect of the stripe being moved on and off the background was absent.

*Experiment 10.* Modified *H.E.* apparatus. Black stripe on white background. The apparatus was modified by cutting a window 13 cm. long, and 8 cm. from the square window. It was thus possible to expose both eyes simultaneously before exposing only one eye.

FIG. 5

FIG. 6

FIG. 7

The procedure was to have the *S* observe the stripe through the long window with both eyes, and immediately afterward to move the disk so that only one eye observed the stripe. Under these conditions I observed no motion. One *S* observed at times slight movement, at other times the stripe did not move. Evidently, if correct convergence is once obtained by both eyes simultaneously, it is easier for one eye to hold it when it is alone exposed.

*Experiment 11. P.E.* apparatus. Black stripe on white background. This apparatus, as was stated above, allows the exposure of part of the stripe to each eye simultaneously. The disk was turned so that the *l* eye saw the top of the stripe, the *r* eye the bottom of the stripe. With the disk at rest, the *l* eye saw the top part of the stripe to the *l* at the same time that the *r* eye saw the bottom of the stripe to the *r*, as in Fig. 5. With very slow movement of the disk there was no apparent movement of the stripe, but the different locations of the stripe to the two eyes was clearly observed throughout the exposure.

*Experiment 12.* *P.E.* apparatus. Black semicircle on white background. The same procedure as in the previous experiment. With the disk at rest, exposing part of the semicircle to one eye and part to the other, the perception was as in Fig. 6. At a certain moderate speed, there was slight movement of half of the semicircle, and the figure tended to complete itself as in Fig. 7. The entire outline of the indented curve was clearly perceived.

*Experiment 13.* In order to examine further the factors connected with exophoria and the imbalance of the ocular muscles, the following procedure was used. Two round holes, separated by interocular distance, were cut in a piece of cardboard. The *l* hole was so covered at the top, and the *r* hole at the bottom, by strips of cardboard that the subject saw the entire upper half of the stripe with the *l* eye and the lower half with the *r* eye. The entire stripe was thus seen, and the perception was that of an unbroken stripe, as would be the case if each eye saw the entire stripe.

*Experiment 14.* *H.E.* apparatus. Black stripe on white background. When the stripe was placed at the distance used in these experiments, I readily noted by closing first one eye and then the other that the images were crossed. The stimulus was now moved gradually away from the instrument. At about 80 cm., for me, the images became uncrossed, which obviously meant that the lines of vision now met in front of instead of behind the stripe. Theoretically, there should be a distance where the lines of vision would meet on the surface of the black stripe, even with alternate use of the two eyes, and where consequently no movement of the stripe would occur. Practically, I was unable to find this point, probably on account of slight shifts in convergence. Observations were made with the stimulus about 2 m. from my eyes. The double images were uncrossed. The movement was of the same nature as with crossed double images, only in the reverse direction. The movement now occurred in the direction of the exposed eye.

*Experiment 15.* *H.E.* apparatus. Artificial pupils consisting of brass tubes, 6 cm. long and 1.9 cm. in diam., closed at both ends and with 1 mm. holes punched in each end. These tubes were inserted in the eye-pieces of the apparatus. The stimulus was the black stripe on a white background. The *S* converged the two artificial pupils on the black stripe until the two fields fused. He then saw a small, round, white field cut in half perpendicularly by the black stripe. Without the artificial pupils, the subjects were unaware of

seeing two fields. The perception was merely that of a black stripe moving across a white background. With the artificial pupils, the black stripe and the small white background seemed to move together from *r* to *l* and the reverse. There was no motion of the stripe relative to the background.

*Experiment 16. H.E.* apparatus. Dark-room. A small box enclosing an electric bulb. In the front surface of the box was a perpendicular slit which could be covered with gelatin films of different colors. White, red, green, blue and yellow films were used. Movement was obtained with all the colors similar to the movement obtained in the other experiments. Good movements seemed more easily obtainable with yellow and blue lights than with red. When the light was dimmed, movement was obtained with flicker effect. Only a few observations were made, but the results suggest further work under more controlled conditions of illumination, speed, etc.

## Summary and Conclusions

It is not the purpose of this preliminary report to offer a theory of apparent movement, but rather to present the results already obtained, some of which seem conclusive, others merely suggestive.

Apparent movement was obtained by the successive stimulation of non-corresponding points of the two retinas. This movement was, in the main, the same as that obtained by successive stimulation of neighboring points of the same retina. The nature of the movement varied with the interval between stimulations. Accurate control of speed was not made, but it was evident that by a change of speed from slow to fast, succession, end movement, full movement, and simultaneity could be obtained. The fusion of the double image into a single stationary image with rapid speed of the shutter is obviously a phenomenon which cannot be obtained by the stimulation of two points of the same retina. Simultaneity was only occasionally obtained, and then after long, continuous observation with rapid speed of the shutter, when the single image broke up into double images, due probably to fatigue of the muscles of convergence. It is a significant fact that the double images at slow speed of alternation gave movement, while at very rapid speed they gave simultaneity.

Under certain conditions, 'pure' movement was obtained. The reports do not throw any light on the nature of this perception beyond the fact that there was a perception of movement without the perception of the moving object. This perception of 'pure' move-

ment was not the perception of movement of the shutter. Paradoxical as it may seem, the perception appeared to be the movement of an unseen object. There was also at times a clear perception of an object moving after it was objectively no longer exposed, and in the direction opposite to that in which it usually occurred when non-corresponding points of the retinas were successively stimulated.

Movement was obtained by the stimulation of only one eye by the object, if the other eye was stimulated before or after by the background. It remains to be determined whether a background differing from the background exposed to the other eye would produce movement. It would also be advisable to have a check upon eye-movements in this experiment. A further result obtained under these conditions was that of the reversal of movement of the object according to the attitude of the subject. The meaning of the total situation changes with a change in the direction of movement. What causes the change in direction is not entirely clear, but it is evident that direction and nature of movement depend upon various factors in the total situation.

From the results of *Experiment 8*, it seems evident that while eye-movements may occur at times, they are not the fundamental cause of the apparent movement produced by the successive stimulation of the two eyes by the object. In this experiment there is the perception of two simultaneous movements in opposite directions over the same area of background. The eyes could hardly move in this way.

The stimulation of non-corresponding points of the two retinas seems to be a necessary factor for apparent full movement, but there are indications that the accompanying degree of convergence must be taken into account. We have the fact that in *Experiment 6* two parallel stripes, one presented to each eye, were seen simultaneously and without movement, while in *Experiment 8* the dot, which was clearly seen to move across the field, stimulated approximately equally non-corresponding points. But in *Experiment 6* the eyes converged very nearly if not on the plane of the stimulus, and the stripes fell on the nasal half of each retina respectively, while in *Experiment 8* the eyes converged some distance behind the stimulus, and the dot fell on the temporal half of each retina.

In these experiments monocular vision was usually employed. The eye rarely, if ever, fixated upon the object. For some subjects, if the object was close the eyes converged behind, and if far away,

in front of the plane of the object.[3] In monocular vision, therefore, the object was seen displaced either to the right or left. If the successive stimulation was very rapid, the conditions became the same as in simultaneous binocular vision, and convergence was presumably upon the object. Under these circumstances, the object was seen singly, it remained stationary, and was located as it would be in Cyclopean vision.

It is apparent that merely simultaneous stimulation of the two eyes is not sufficient to overcome exophoria by inducing the adequate convergence reflex. When the upper part of the stimulus fell on the one retina, and the lower part on the other, the perception was comparable to that of a double image (*Experiment 11*) even though the stimulus fell on the two retinas simultaneously. When, however, convergence would bring about the perception of a completed figure, (in this case, the entire stripe—*Experiment 13*) convergence was directed and held on the object, and single image occurred. Under this latter condition, the stimulation of the retina of the one eye continued perpendicularly to a point where the stimulation of the other eye began. That is to say, if the two retinas could have been placed one over the other, contiguous or almost contiguous points would have been stimulated. There would have been no appreciable gap.[4]

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[3]See C. H. Judd, Binocular factors in monocular vision, *Science*, N.S., 7, 1898, 269 ff. Judd states that the images are crossed when the object is at a distance. With four of the *S*s of our experiment, this was the case. Two *S*s, however, obtained uncrossed images when the object was at a distance of 5 m.

[4]Judd writes, "It is undoubtedly true that the retinal sensation is in a certain sense the controlling factor in the total binocular fusion. It is only when like stimuli act upon the two foveas that the eyes can come to rest and the converging of the lines of regard as a condition preliminary to holding the two eyes fixed upon a certain point in spite of their natural tendency to move together in a lateral direction, is undoubtedly related to the superior significance in experience of the retinal sensation from the two foveas." Records of convergence and divergence, *Psychol. Monog.*, 8, 1907, 414. In our experiments, the fact of the stimulus falling on the two foveas did not bring about adequate convergence unless the stimulus fell upon correspondingly contiguous points. Judd, however, is probably correct when on page 415 of the same article he states that "for the more elaborate co-ordination, higher centers must be drawn into action, and correspondingly a higher form of experience must be expected," since the factor of the perception of the figure as a whole may well be involved in the development ontogenetically of the functional relationship of contiguous retinal points.

# THE RHYTHMICAL EXPERIENCE FROM THE SYSTEMATIC POINT OF VIEW

By CHRISTIAN A. RUCKMICK, University of Iowa

Of all the newer developments in the field of general systematic psychology during the last decade probably the most influential in the direction of the revision of our fundamental concepts is the movement best known as *Gestalttheorie*. While this theory has been applied to many phases of mind, to say nothing of physical and physiological conditions, it has found its most fruitful application to perceptual processes of a visual type. Lately, however, the data and theories connected with auditory processes have been brought under the scope of this movement as a number of recent researches will attest.[1] This is of special interest in view of the fact that one of the earliest

[1]M. Eberhardt, Über Höhenänderungen bei Schwebungen, *Psychol. Forsch.* 2, 1922, 3363-45. The investigator here discovers that the pitch of the beating complex is conditioned by the relative intensity of the two generating tones. No single pitch resultant can therefore be isolated without taking into account surrounding conditioning factors. In a subsequent research (Eberhardt, Über die phänomenale Höhe und Stärke von Teiltönen, *Psychol. Forsch.*, 2, 1922, 346-367) concerning partial tones the same investigator finds that both central and peripheral factors are operative and that it is difficult to analyze out the separate influences. She makes the very interesting point that clangs are judged to be pure until the attitude is changed to that of analysis. In somewhat of a similar way Von Hornbostel also calls attention to the central factors in dichotic hearing (E. M. v. Hornbostel, Beobachtungen über ein- und zweiohriges Hören, *Psychol. Forsch.*, 4, 1923, 64-114). The influence of each ear separately in addition to the central factor can not be mathematically determined, the whole forming a single pattern or form. Kester has also approached the problem of sound localization with much the same result as far as pattern is concerned (P. Kester, Über Lokalisations- und Bewegungserscheinungen bei Geräuschpaaren, *Psychol. Forsch.*, 8, 1926, 75-113). Helson in his summary of the whole field of *Gestalt* refers to the amount of work done in the field of auditory sensation (H. Helson, The Psychology of *Gestalt*, *Amer. J. Psychol.*, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-223; cf. especially 37, 1926, 29-33). Köhler himself makes a point of certain errors into which psychologists have previously fallen in connection with successive or simultaneous comparisons (W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 115-175; cf. especially 131 ff.). Lewin brings the subject to bear more definitely on the total physical situation which gives rise to interference tones. He makes the point that each separate interference-tube contributes to a closed system where even the spaces between interference-tubes must be taken into account (K. Lewin, Über den Einfluss von Interferenzröhren auf die Intensität obertonfreier Töne, *Psychol. Forsch.*, 2, 1922, 327-335).

intimations of the *Gestalttheorie* as given by Von Ehrenfels was in terms of the peculiar structure of the musical chord and in the possibility of transposing the chord.[2]

The two outstanding characteristics of the pattern or form, namely, its uniqueness and its transposability, can be applied *pari passu* to melody. In the melody there is a unique character introduced through the interrelation of the notes, and the melody or theme is recognized as such regardless of its position on the staff; or stating it another way, the melody, theme, or tune is recognized regardless of the type of voice or instrument which announces it. Of course, this means that melody as such possesses features which the individual notes lack. Stated in terms of mental processes, as many psychologists have noticed, perceptually and conceptually the harmonic effect of the chord and the melodic progression of a theme are unitary.[3] It must also be noticed that the usual modifiability of perception occurs here as elsewhere. Certain slight changes in the physical configuration of the melody or of the chord will pass unnoticed in the perceptual process. Melodies may be slightly changed up to the breaking point without interfering with the recognition of the melody as such. Moran and Pratt have recently shown that the same modifiability can take place in interval relationships without altering the perception of the interval.[4] Of course in all of these cases there may be large individual differences.

It is important to note here that as Stumpf has already pointed out in his theory of the origin of music both the perception of consonance and melody and the ideation of them represents a stage of development of the human mind.[5] In the lower forms of animals the perception of the tonal effects as such, or of the melody, would not be possible. Especially when the ideational type of mental processes first dawned in the history of the mental life both consonance

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[2]C. Ehrenfels, Über "Gestaltqualitäten," *Vjsch. f. Wiss. Phil.*, 14, 1890, 249-292. There are two factors which Ehrenfels emphasizes in this connection: (1) that the chord itself had characteristics not assignable to any one or more of its parts; and (2) that it could be transposed from one position on the staff to another without losing the essential characteristics of that chord. See especially p. 552 in Koffka's review of perception (K. Koffka, Perception: an introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 531-581).

[3]C. Stumpf, *Die Anfänge der Musik*, 1911, 28 ff.; W. V. D. Bingham, Studies in melody, *Psychol. Monog.*, 12, 1910, no. 50, 1-14.

[4]H. Moran and C. C. Pratt, Variability of judgments on musical intervals, *J. Exper. Psychol.*, 9, 1926, 492-500.

[5]Stumpf, *op. cit.*, 23 ff. Stumpf notes in this stage of development the ability to abstract (*Fähigkeit der Abstraktion*).

and melody took on new possibilities. Their effects could not only be experienced at the moment of presentation, but they became at once part and parcel of the equipment of the individual for purposes of memory and recall. In the mind of the composer and of the highly cultured individual from the musical point of view, ideated chords and melodies form the vocabulary of the musical language. The mind of a great conductor or composer of music is usually found to be well stocked with thematic and chord material ready to be utilized in the form of new compositions just as a comprehensive vocabulary together with happy phrases form the building stones of the writer or speaker in the literary arts and humanities.

While this aspect of pattern or form has received emphasis in our discussions aside from content, a careful scrutiny of the situation in terms of *Gestalt* as outlined by its chief proponents will reveal the important fact that content itself must not be overlooked as a conditioning factor. Although melody as such can be conceived independently of its constituent parts and is not merely an ideation or juxtaposition of these constituent parts, the pitch at which it is sung, the important variations that occur in its form when it is transferred from a major to a minor key, or even when its parts are only slightly altered without disturbing the essential thematic material—all or any one of these may become influential factors that complete the total configuration. They furnish the setting, the background, the atmosphere of the whole and, while even with extensive expert training we may not notice the variations in the material presented, we must not forget that there are often physiological or unconscious factors at work which produce their effect through determining tendencies and 'cortical sets.' All this applies with equal force to the chord as, for example, the C-major triad. Establishment on an octave higher or lower in pitch or a change of a few vibrations according to the pitch system that is used does not prevent the recognition of the chord as the triad in C major. But the change of an octave makes a difference.[6] Part of this is due to the fact that the actual tonal mass physically changes. Not only have some of the lower tones been eliminated but some of the higher ones

[6]Of course, in the case of absolute pitch consciousness which may be considered as an anomalous condition, the individual is very much disturbed if the chord or melody has no constant relation to a very definite note so far as his memory is concerned.

as well, because in the system of partials the upper end of the series has gone beyond the tonal limit of hearing. Part of the effect lies in the total aesthetic picture presented.

All of this has a direct bearing on rhythm. In music the melody may be considered as a pattern of two dimensions, pitch and time. The chord adds to this the third dimension of depth in terms of tonal mass or musical 'volume,' while rhythm provides the repeated periodic accent which adds movement to the whole. Just as melody and harmony can be conceived as independent of the content but nevertheless in a certain sense conditioned by the content, so rhythm can be considered in its mere formal aspect as separate from materials out of which it is composed. And yet, as many investigators have shown, the form of the rhythm is itself conditioned to some extent by the content. Woodrow has not only investigated the relative effects of the attributes which condition rhythm,[7] but has also determined quantitatively the "breaking point" of the rhythm when one factor operates against another.[8]

There is one particular feature of the rhythmical experience which ought to be observed here. When we come to this form of experience we recognize that rhythms can be perceived in sense departments other than the auditory group. There has been considerable discussion on this point in view of the fact that rhythms are so frequently connected with music where the sensory materials are auditory sensations. But as the writer has himself discovered and as other psychologists have pointed out, rhythm can be touched off by visual sensations and tactual sensations.[9]

Considerable interest has centered around the function of kinaesthetic sensations in the rhythmical experience. Undoubtedly there can be pure motor rhythms without visual, auditory, or kinaesthetic accompaniments. But the motor factor is an almost omnipresent one in rhythms of all sorts of sense material presented. In his own intensive investigation[10] of the subject the writer's interpretation may be restated in the light of more recent investigations somewhat

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[7]H. Woodrow, Quantitative study of rhythm, *Arch. Psychol.*, no. 14, 1909, 1-66.

[8]Woodrow, The rôle of pitch in rhythm, *Psychol. Rev.*, 18, 1911, 54-77.

[9]C. A. Ruckmick, Visual rhythm, *Studies in Psychology, Titchener Commemorative Volume*, 1917, 231-254; C. A. Ruckmick, A possible interpretation of the synchronous flashing of fireflies, *Trans. Ill. State Acad. of Science*, 13, 1920, 109-122. The work on tactual rhythm is still in manuscript form.

[10]Ruckmick, The rôle of kinaesthesis in the perception of rhythm, *Amer. J. Psychol.*, 24, 1913, 305-359.

as follows. It is very likely that in primitive surroundings rhythm was largely motor in composition. According to theories which connect rhythm with coöperative labor, such as rowing and heaving, the periodicity which became experienced was largely of a muscular type. In ceremonial activities the dance played an important rôle and the muscular contractions of singing and of playing rudimentary musical instruments must not be overlooked. In the latter case, of course, they were combined with auditory material just as in the former case. If the dance were a silent affair, as it very often was not, the visual sensations aroused might be combined into one unit. In the present perception of rhythm it is not amiss to think of these motor contractions as traces of this original manifestation. Rhythms were originally built around the motor aspect of mental life. They are still built to some extent around this factor. Another way of looking at the same phenomenon from its reversed aspect, as it were, is to say that many of our perceptions tend toward a motor expression and Bingham himself has attempted to interpret melody in this wise.[11]

We have all experienced the unquestionable situation involved in trying to inhibit natural rhythmical contractions of the body. When beginners first learn to play or to sing musical compositions there is an invariable tendency to move some part of the body rhythmically in addition to those parts directly affected by the rendition. When a stirring march is played we can hardly restrain ourselves from tapping the foot or moving the hand or producing some other perhaps less visible contraction. But as the process becomes more and more abstracted and rhythm is experienced as an idea rather than as a perception, the kinaesthetic factor gradually disappears. When a complex rhythm that is presented either auditorially or visually is once recognized, the kinaesthetic factor is found slowly to vanish. Pure auditory, visual, and tactual rhythms have been thus obtained. So, also, in expert musicians who have mastered many forms of rhythm and can ideate them readily from the perceptual cues in the score the motor expression is no longer a necessity. Kinaesthesis has performed its function long ago and has either been consciously inhibited at some stage of training or has naturally passed away. The writer has consulted many musicians on this point and finds that his observation is true. As in many other types of learning the

[11]Bingham, *op. cit.*, 81-88.

amount of motor energy expressed in mastering a situation is gradually reduced in actual performance. Rhythmic expression is no exception to this rule; that is, the energy entering the sensorium is transferred with little loss at this time to the specific motorium and becomes centered there. In a skilled performance the motor factors even in the members of the body involved become reduced to a minimum. The point that the writer is attempting to make here goes one step farther. Rhythmical expression like every other expression has been strongly weighted with motor factors from early primitive times. While rhythm at this level of mental development likely is very strongly motor, with the greater predominance of conceptual life in advanced stages of civilization the motor factors themselves may drop out as inhibiting factors set in.

Here, of course, we must recognize many factors which vary the gradual decadence of motor concomitants. Certain races tend strongly toward motor expression as, for example, the peoples of Latin origin. Besides there are traditional influences at work so that in the highly restrained life of our cities there is much less expression of emotional and perceptual processes than one finds in the more naïve and unrestrained minds where everyday life is not so directly contingent upon the life of the rest of the community. There is, of course, an individual variation based upon inheritance or specific training. The finished and accomplished musician very often has not only confined his expression to very definite patterns but reads and thinks about music in the abstract. Meanings need no longer wait for definite motor expression before he can comprehend them and rhythmic meaning is no exception to this rule. The merest vestige of some kind of kinaesthetic twitch may be all that is left; but also introspection reveals that even this is wanting and in its place there is some bit of visual or auditory imagery which vicariously functions in its place just as in the case of some pronounced musicians the situation is reversed in that auditory imagery is entirely replaced by voco-motor reactions. The tendency, however, in ideational processes and in most individuals who think of music in a rhythmical way is for the kinaesthetic factor to become eliminated and, while the meanings may have been kinaesthetic in origin, they may now be comprehended and ideated through other mental processes such as vision and audition. In a few cases even organic sensations seem to round out the meaning.

A factor which has often been mentioned in connection with rhythm is that of attention and we now face the problem of what is the relation between rhythm and attention. As the writer has already pointed out in another study[12] the organizing aspect of attention comes to the front in what is usually known as subjective rhythm. The presentation of totally disorganized heterogeneous stimuli soon is brought into a semblance of order and later into more or less perfect order through the influence of the perceptual process itself. Ten metronomes going at different rates are perceived very soon in the form of a regular rhythm. Knowing these conditions the writer has applied the organizing principle of attention to visual stimuli in his interpretation of the alleged synchronous flashing of fireflies.[13] Sometimes a lead is given the attentive function through accidental concomitance of stimuli or at least by the concomitance of a major portion of the group. This becomes impressed and predisposes mind to the experience of a definitely repeated pattern. In the beginning of the experiment just referred to a visual rhythm was suggested for a few measures at the opening of the series on the part of the majority of the twenty points of light. The stimuli which followed were physically totally disorganized, but became readily organized in experience. Of course there need not be actual physical concomitance. Slight temporal separation under favorable conditions, as *Gestalt* psychology has pointed out, would lead to a unitary perception just as it does in sound localization and in the work and experiments of the two-point limen on the skin.

In addition to this general organizing principle which continues periodically a pattern once set, there must be recognized, of course, the influence of the attentive function on the attributes of the processes themselves, a factor which is already presupposed in the previous discussion. The durational attribute of the members of the rhythm as well as the intensive attributes, and on some occasions even the qualitative aspects, are changed. In the experiments with the visual stimuli in connection with fireflies it was often reported that the color of the light which was accented would change from yellowish white to blue. Of course the raising of the pitch of the 'tick' in the repeated tick-tock of the metronome is a case in point in the auditory field. In both the visual and the auditory series there

[12]Ruckmick, Rhythm and its musical implications, *Proceedings M. T. N. A.*, 1924, 53-62.
[13]*Op. cit.*, *Trans. Ill. State Acad. of Science*, 13, 1920, 119 f.

was no physical change in quality. Even experienced observers will report the subjective action of attention. In connection with the attributes of the members we often find observers who insist that the more intense sound on the Seashore record for the testing of musical talent[14] is higher in pitch. This in the writer's judgment is one of the factors at work in Woodrow's experiments in which the relative distance of members in the series, *i.e.* the distance between members of the same measure *vs.* distance between the last member of one measure and the first member of the next, was used to cancel the intensive and durational aspects of the members themselves. Undoubtedly without the capacity of the human mind to relegate objects and events to the foreground and background of the mental life, perception itself would not be possible. The psychology of form, of constellation, of organization presupposes the working over of materials from the mental point of view. This factor has been traditionally studied under attention. Whether the term attention will remain in our scientific vocabulary may be much doubted since it is distinctly a functional and therefore teleological concept; but the facts, especially in rhythm, are clearly before us.

We now come to perhaps the most important question in the psychology of rhythmical perception. What is the nature of rhythm? In part this has already been answered. By bringing together the various threads of discussion in the previous sections we can obtain a comprehensive answer to the question. But a succinct formulation is always helpful. Rhythm in whatever sensory field it occurs may be defined as *the perception of a temporal form or pattern in which individual members repeated periodically are consistently varied in any one or more of their qualitative or quantitative attributes.* As in many other explicit formulations the statement must not be torn from its context or misinterpreted in terms of any other type of exposition. The writer does not mean that in the pattern or form the members are recognized as such. Indeed, he has done everything to show that in the perceptual and especially in the conceptual process the items or members may be partially submerged or relatively unclear, or in some cases there may even be unconscious or physical effects. When the rhythm of a dance is quickly grasped as soon as the music has begun the writer doubts very much whether in the highly rhythmical individual the single notes stand out in consciousness at all. He is

[14]C. E. Seashore, Measures of musical talent, *Record No.* 53003-*D*, *Columbia Phonograph Co.*

much more inclined to believe that in many cases the effect is of a physiological sense order. He is not willing to go as far as Hönigswald does in interpreting rhythm as "funded elements."[15] The pattern of the rhythm may be as directly and purely perceived as the clang, noted above, before it was analyzed. Here the concept of *Gestalt* or form proves to be very helpful. Both in the percept and much more in the concept an abstract meaning has occurred which has the barest sensory foundation. Thus, a skillful musician on the one hand, or a skillful dancer on the other, recognizes a rhythm very much in the same way as a person does who glances at his watch and notices the time without being immediately able to tell what time it was. In the same manner a person may be able to read a foreign language without being immediately able to translate the meanings into English words. In association tests very often we come upon people who have visually associated situations with each word, each one of which must then be translated into verbal terms. In rhythm, therefore, the sensory cues are relatively unclear and the main process attended to is the rhythmical unit itself. This is especially true when rhythms are completely recognized and of course does not hold, as we have indicated before, for rhythms in the process of learning where kinaesthetic factors as we have noted are especially permanent.

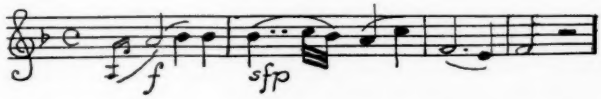
In other words when an expert musician reads a musical passage the rhythm is comprehended as a meaning of its own without very much consideration of the tones which make up the rhythm. A skillful reader of poetry in the same way catches the meter without much attention to the separate words involved. The writer has often observed both classes of readers and talked with a number of them in order to get their analyses of the rhythmical experience. With rapid readers of both types who have become skillful in their art there has never been any question of analysis in either case. The form or *Gestalt* is immediately given. We thus see that not only the motor factors as such but at least attentively speaking the sensory factors may also almost entirely disappear. A mere vestige of sensory cues remains. Putting it in another way, the type of the rhythm claims primary attention while the factors which produce the rhythm are in the background and sometimes probably unconscious.

The question of the inheritance of rhythm which has been discussed by Swindle[16] and others resolves itself into the following.

[15] R. Hönigswald, *Vom Problem des Rhythmus*, 1926, 26.

[16] P. F. Swindle, On the inheritance of rhythm, *Amer. J. Psychol.*, 24, 1913, 108-203.

Genetically the human mind has developed to the point when it is possible for it to ideate auditory and other patterns in such an abstract way that the factors which produce that pattern or form have dropped into the background of experience. Such a pattern as a rhythmical unit may then on subsequent occasions be ideated in memory, imagination, or thought in terms other than the original contents. The images which form the background of these forms may even suffer transformation and a certain amount of elimination. Only in that form of mind which is capable of abstract ideas can rhythm be recognized as mere form. In other types of mind, as in the famous case of Jacques-Dalcroze's student, the rhythm must be actually lived out through expression before it can be realized. As will be recalled this student could not follow an auditory rhythm in music without stepping out on the carpet with her feet the rhythm which corresponded to the musical structure. There is actually, then, nothing in the inheritance of rhythm except the inherited capacity of the human mind in the first place to organize discrete stimuli into higher patterns, to cognize these patterns as such, and later on through ideational processes to recognize them or to use them ideationally in any of the higher mental processes.

We close then with this general conclusion. The perception of rhythm offers no exception systematically to the general classification of perception where form and not content is emphasized. Experimentally it is not possible to divorce the melodic form, the chord form, or the rhythmical form from the actual contents. The contents and their surrounding conditions modify, even if ever so slightly, the form itself. A rhythm may be of a certain typical structure, as for example:

and still be recognized as a definite form of rhythm regardless of its position on the staff or of the exact pitch of each note. And yet just as in the other instances this rhythm is not independent as a whole of all conditioning factors which affect it. Incidentally this forms the writer's main criticism of the *Gestalttheorie*. Even if these forms are accepted as the successors to mental processes considered as elements, they themselves are subject to conditioning factors which

surround them. Analysis anywhere along the line of mental processes will always remain, under the category of a science, an artificial procedure. Just as we can not absolutely tell the chord apart from its actual pitch relationships, so in this instance we cannot summarily remove the rhythmical pattern from the melodic and harmonic relationships that surround it. Putting it more tersely the rhythmical pattern as it is announced by the tympani in an orchestra is actually not the same rhythm that is announced in the melodic passage of the strings. But since attention for the moment centers chiefly on the temporal pattern of the rhythm as a unit we ordinarily neglect for the most part these conditioning factors. An elaboration of this thought means also, for example, that a rhythm perceived in any one sensory field like the auditory field is not exactly the same in all of its relationships as the same temporal form that is perceived in some other sensory field. Strictly speaking, an auditory rhythm announced by a musical instrument is not the same rhythm that is visually presented in the score.[17] Thus the rhythmical experience systematically considered through the fact of attention and the general organization of mind is simply another instance of the immediate perception of form which is not wholly dissociated from the constituent parts which establish the rhythm.

[17]It therefore follows that the reading of rhythms from score can not be systematically identified with rhythms that are heard from musical instruments. This opens up a long avenue of experiments on what actually occurs in the reading of rhythms from score and their concomitant production through musical instruments. For certain purposes, of course, as the writer has pointed out, cross-identification is possible because attention centers itself mainly on the form, but systematically a slight difference in the conditioning factors must occur.

# THE PSYCHOLOGY OF FELT EXPERIENCE

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## INTRODUCTION

Since the discoveries of spots of cutaneous sensitivity by Blix, Donaldson, and Goldscheider, the psychology of felt experience has been dominated by two theories. The first of these theories posits 'spots' of sensitivity with a separate nerve supply for each type of experience, such as the sense of pressure, sense of pain, etc. The second posits the differentiated nerve-endings of the skin, Krause end-bulbs, Pacini corpuscles, etc., as the separate receptor arrangements demanded by the first theory. For about forty years these theories have been widely accepted, much worked upon and without progress. They are today practically where they were in the days after the first publications, although they have drawn to themselves the efforts of such men as Von Frey and his laboratory beside those first named.

The particular facts of these theories to be noted are the very unstable nature of the spots, when found, and the complete failure of the attempted correlation between the spots and any of the differentiated structures. The only notable change in the usual presentation of the subject is the tendency shown in recent years to abandon the numerous sense modalities once posited in this connection, such as cutaneous sensitivity, kinaesthesis, joint sense, muscle sense, and to unite these modalities into one sense department recognizing pressure, warmth, cold, etc. as 'qualities' of the modality.

The very ease with which this change has been effected suggests the lack of any adequate criterion by which to classify. As a matter of fact, the number of qualities accepted by any given individual is usually greatly influenced by the number of differentiated end organs he believes are likely to prove available for purposes of correlation.

Regardless of the accuracy of these theories or the lack of it, whatever advances the psychology of feeling has made in recent years have been without aid from the theories and rather in spite of them.

## THE PRESENT STUDY

The problem set here is to look into the 'simple' nature of the qualities of felt experiences posited as 'unanalyzable.' In two previous problems we found that bright-pressure and dull-pressure are analyzable and that tickle and itch are rather easily analyzed.

Our list of qualities is taken from Titchener. We have found that these terms are not universally used alike and so, in spite of making the description redundant to some, we have defined all of the terms used as Titchener defined them, either in print or in his lectures. We then present our analysis of these 'qualities' together with facts reported in problems published by others.

We find only one quality in felt experience, the quality of brightness. The dominant brightness is patterned, however, and the degree of brightness, together with the pattern of concomitant brightnesses, distinguishes the particular experience, much as pitch and timbre distinguish an auditory experience.

The terms used to describe other aspects of these experiences are voluminous or small, loose-textured or dense, diffuse or well-defined, flat or penetrating. So far as the present problem is concerned these are not shown to be independently variable, the duller experiences being always voluminous, diffuse, loose-textured, and flat, while the bright experiences are always small, compact or dense, well-defined, and sharp or penetrating. We did not, however, attempt any study of the independent variability of these aspects.

The patterns show a greater or less degree of complexity, the experiences of warmth and cold, especially, proving to be relatively simple and difficult to analyze. Others yield more easily to analysis. Some patterns, relatively gross, have been previously analyzed, such as Bentley's 'liquidity' and Zigler's and Meenes' 'roughness' and 'smoothness.' These showed patterns of temperature and pressure and patterns of separate (more or less fused) pressures, the simpler patterns within the grosser structure maintaining their identity like the notes of a chord. All such patterns of felt experiences ordinarily are described by giving a general name and the conditions under which they occur, *e.g.* the stickiness of glue, the heat of an oven, the prick of a needle.

The similarity between these facts and those of audition is striking, and some of these similarities are presented.

Titchener's 'touch pyramid' is discussed in the light of these findings and is found to be inadequate to the facts.

In the work here reported we have attempted to determine some of the facts of the psychology of felt experience independently of any theory and for the present we do not attempt any systematizing of

our facts. At some later time we hope to present a discussion of facts and theories in monographic form. The literature is at present much too large for treatment in a short article.

We review some of the facts of affective and emotional experience, which we regard as patterned feelings in the same sense as the simpler experiences dealt with earlier in the paper. We also discuss, very briefly, the confusion of terms which has resulted from the current classifications of felt experiences and conclude that cutaneous sensitivity, kinaesthesis, organics, affective pleasantness and unpleasantness and all of the emotional experiences are patterns of felt experience and belong within one general modality, the proper name for which is the sense of 'feeling.'

## Titchener's Simple Qualities of Touch[1]

As a place of beginning we accept Titchener's qualities of 'touch.'

*Tickle.* This word is used in its common meanings. It appears that Titchener does not distinguish between cutaneous tickle, such as that aroused by brushing a feather lightly over the skin, and the emotional upset sometimes produced, especially in children, by lightly poking the ribs, etc. The one is simpler than the other but apparently the two experiences are qualitatively identical. The kind of tickle found at the apex of his pyramid and discussed below is cutaneous tickle (157–159).[2]

*Itch.* Itch is differentiated from tickle as being a light pain whereas tickle is light contact (157–159). This has not met with general acceptance; and itch, as a cutaneous quality, has no standing at present outside of Titchener.

*Contact.* Contact is the weak, thin, wiry, bright experience aroused by flicking a single hair, or by such light contact as that of the very slight touch of the end of a hair against the skin (146). It is, for him, weak pressure, but this probably means only stimulus-wise, not psychologically. He nowhere states a qualitative resemblance between pressure and contact except that he places them near together on the pyramid.

*Pressure.* Pressure is the sensation aroused by pressing (*e.g.* a horse hair) on a pressure spot, where a pressure spot is defined (over a hair bulb) or found by trial. In some cases, at least, it must be found (146 ff). More grossly it is also the feeling aroused by touching or being touched by an object where it may have to be analyzed out of a complex including other sensations, *e.g.* warm and cold. The same quality, which he often refers to as 'neutral pressure,' may be found in sensations from muscles, organs, etc.

*Prick.* Prick is the sensation aroused by the application of a needle to a pain spot. Pain spots are not defined but must be discovered.

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[1]Touch is here, for him, a general term including the cutaneous, kinaesthetic, and organic sensations.

[2]The page references are to E. B. Titchener, *A Text-Book of Psychology*, 1909.

*Clear pain.* Clear pain is not well defined for us. It is evidently all pains which are not prick and not quick pain; but he has stated that prick and pain are qualitatively identical.

*Quick pain.* Quick pain is derived from the quick beneath a finger-nail, the dentine of the teeth, the pricking of the external auditory canal and perhaps a few other places.

*Strain.* Strain is his former 'tendinous sense' but later was made to include many pressure-like experiences such as that aroused by contracting a muscle firmly but not vigorously (163).

*Drag.* Drag is his earlier 'muscle sense,' a 'dull, dead, diffuse,' experience; like fatigue or over-strain (162).

*Ache.* Ache is used about as in common speech. It is an 'intensive drag,' a 'dull pain' (162).

*Cold.* Cold is the sensation aroused by the stimulation of a cold spot, which cannot be defined but must be found by trial. It is a bright, compact experience, etc. (149-152).

*Warmth.* Warmth must also be defined by method of arousal.[3] He has described it. It must be distinguished from heat.

*Heat.* Heat is the experience aroused by the stimultaneous stimulation of warm and cold spots. It is like pressure and like prick or pain. In his theory for ordinary experiences of heat, Frey's 'paradoxical cold' plays an important part.[4]

*Bright-pressure.* Bright-pressure and dull-pressure are the two of Titchener's qualities least known. He describes bright-pressure as a diffuse bodily feel, bright, bouyant; an experience that goes with a general feeling of well-being. It is not described as containing an element of pressure, pressure being, in this instance, a general term.[5] The experience could as well have been called a 'felt-brightness' and it does not necessarily occur so diffused as to include the whole body.

*Dull-pressure.* Dull-pressure is a diffuse bodily feel also, but duller, heavier, more as one feels the day before one comes down with influenza, yet not strong like drag. These terms came into use with a fairly large group but have never been generally accepted.[6]

It is not the longest list, *e.g.* Ward's would furnish more qualities, nor is it the shortest; but it is fairly representative and much work has already been done upon the qualities there represented. The

[3]But Titchener expressed a doubt as to warm spots in 1903. Cf. *Amer. J. Psychol.*, 14, 1903, 453.

[4]This in spite of the fact that in his announcement of the discovery of paradoxical cold, Von Frey states that it is a rare experience, difficult to produce and usually or often does not occur at the cold spot. Cf. M. von Frey, *Ber. d. kgl. sächs. Ges. d. Wissensch. Leipzig, math.-phys. Classe*, March 4, 1895, 172.

[5]The confusion of the terms touch, pressure, and feeling, is treated later in this paper.

[6]In a previous article by the present author, An experimental study of the affective qualities (*Amer. J. Psychol.*, 25, 1924, 507-544), the terms 'bright-pressure' and 'dull-pressure' were extensively used. The assumption that the terms were generally understood proved to be a grave mistake. They originated with Titchener evidently about 1919 and are familiar only to Cornell students since that time and to a few others who have inquired about them.

qualities are here defined, not for completeness but just enough to avoid misunderstanding and, as nearly as possible, as Titchener defined them.

## Experimental Analyses of the Alleged 'Simple Qualities'

*Tickle, itch.* The research on tickle and itch has been published separately.[7] These two experiences were found to be identical in quality, the only observable qualitative aspect being a 'brightness' identical with the brightness of contact. Murray had previously found the same thing.[8] The experiences are also described as extended, attention-demanding, reaction-demanding, and have a characteristic temporal course involving movement, flashing or irradiation. Localization is fairly definite. The experiences are distinctively patterned but not all patterns are identical. No distinction was found between tickle and itch; although some *O*s have since then given one based upon the attensive aspect.

*Bright-pressure, dull-pressure.* Bright- and dull-pressure are also described in another paper.[9] It was there found that the only qualitative characteristic is, in the case of bright-pressure, a brightness. The whole experience is highly patterned and the patterns vary to some extent from one experience to another. The experience, as a whole, is found to be diffuse and volumic and within the pattern the brightness consists of discrete points, as in tickle and goose-flesh. The experiences are localized[10] and are not attention-demanding; *O*s must be instructed for them or they are not reported.

In the experiences of dull-pressure the quality is 'duller,' 'less bright,' 'dull,' 'massive,' 'like pressure,' etc. It is volumic and diffuse but less so than bright-pressure. The patterns in which the experiences occur are characteristic but not identical. The experience is not attention-demanding for itself.

*Contact.* Our *O*s found contact to be qualitatively bright, less so than prick or cold (when aroused by point stimulation), not as lively

[7] J. P. Nafe, Dermal sensitivity, etc., *Ped. Sem.*, 34, 1927, 14-28.

[8] E. Murray, A qualitative analysis of tickling, etc., *Amer. J. Psychol.*, 19, 1908, 289-344. Murray finds the qualities of tickle and contact to be identical and qualitatively bright. She suggests a brightness series for touch experiences and makes a few analyses in this and a succeeding article, Organic sensation, *ibid.*, 20, 1909, 386-446.

[9] J. P. Nafe, An experimental study of the affective qualities, *Amer. J. Psychol.*, 35, 1924, 507-544.

[10] In the problem referred to the *O*s did not localize the experiences well, but they were not especially instructed upon this point. A paper upon this particular problem, about to be published, will bear out the statement.

as prick but not entirely quiescent. It is a very thin, meager experience: "a small drop of brightness, compact, well defined and fairly well localized." It is not attention-demanding; on the contrary, it is seldom reported unless the *O* is instructed for it. It is a relatively simple experience.

*Prick.* Again there was but one quality reported, that of brightness, like the brightness of contact, cold, or pain, but brighter than the two former experiences and not so bright as the latter. It is similar to another experience commonly called 'sting' but differs from it; and there are other experiences similar to both prick and sting: "Bright but too weak to be called pain. It is like a warning, not painful but it may be soon." Attention always goes to it at the beginning of each experience but does not remain as steadfastly as in pain. There are marked tendencies to reaction; but these disappear as the *O* becomes accustomed to the situation, that is, as the course of the experiments progresses. The experience is described as well-defined, dense; and it flashes out more pronouncedly than with cold or contact but less so than with tickle and pain. There are many references to intensity but greater intensity seems to mean greater brightness, greater density, more 'lively,' etc. We used a needle as stimulus, slightly pricking the skin; but the same sort of experiences may be aroused by the use of a blunt pencil point in mere contact with the skin.

*Pain.* For the greater part we used only one experience for pain. Seven of our *O*s, during the course of this investigation, were having teeth filled. The contact of the dentist's drill with a sensitive spot in a tooth was taken as an example of pain and the *O*s were instructed to report the experience. Pricking the external auditory canal was also used for purposes of comparison. Other scattering examples were used and will be described as mentioned.

The reports of the seven *O*s were almost identical. There were two qualities reported: an extreme brightness, mentioned by all *O*s; and an extreme cold which accompanied the pain, reported by one *O* twice in five reports. The reports of brightness are usually couched in superlative terms; 'extremely bright,' 'excruciatingly bright,' 'a hard, sharp, shooting brightness,' etc. All reported the experience to be extremely attention-demanding. Other experience is "lost sight of." Even a wedge driven between two teeth, "which was, a moment before, very painful itself, was completely lost during the experience of pain." All reported kinaesthesis present before and after the pain,

especially before; muscular movement, gripping of the hands, general bodily strain, etc. There were also evidences of such things occurring during the pain, *e.g.* one *O* so tightly gripped his hands that the nails of his fingers drew blood from his palms but he was not conscious of any experience from that region until after the drill had been removed.[11] The pain experiences are also reported to be very dense, hard and very fine. "Like a live wire but dense like glass." "Very fine and extremely dense." The experiences are well localized at first but when continued or persistent the localization becomes wavering. The pattern is well-defined and very lively. There were reports on intensity similar to those for prick in their references to greater brightness, etc.

The pain from the ear was described in similar terms; but such pains were stated to be qualitatively easily distinguishable from those of the teeth. A 'smarting pain' which resulted from a cut was also reported in similar terms but was described as having elements both of heat and warmth and was stated to be distinguishable from the pain of the teeth and ear.

*Pressure.* This is common pressure or 'neutral pressure.' We used as stimuli a piece of leather at about skin temperature, and metal cylinders at about skin temperature, both of which were brought into contact with the skin. For active pressure the *O* was instructed to touch leather and wood at about skin temperature. We also used movement of the arm, finger and tongue. All reports of quality were of dullness or comparative dullness or neutrality. "Qualitatively there is not much to talk about. The experience is there, big, dull, not dull in the sense that ache is dull, I should say that neutral is the best word for it, although it certainly is dull compared to some of the other experiences." It is usually described as 'dull' in these experiments. In another series, when working on temperature the experience at skin temperature is often called 'neutral.' The experience is well localized but not well defined, the edges have no definite place. Attention goes to the experience as it begins but is not held, as in the case of many of the brighter experiences. It is more volumic and more diffuse than any of the other experiences described here with the possible exception of warmth.

[11]We did not attempt any study of the emotional aspect of these experiences of pain.

The different types of pressure experiences were reported to vary sufficiently to be recognized as different yet all were described as of the same quality and generally alike.

For strain we used contracted muscles of the arm and tenseness of the whole body. The reports identified the experience, qualitatively, with that of pressure. These are more extensive, involve the body more and, in the case of the general bodily strain, were described as "general, diffuse, bodily pressures." Attentively these pressures might be consolidated as a general bodily pressure; but attempts to localize found the pressures to be separate and to come from some parts of the body and not from others.

*Drag and clear pain.* We were not able to produce drag and clear pain.

*Ache.* We produced ache by intensive physical pressure over a small area of the arm and by plunging the hand into ice and water (salted) at about $-10°C$.

The latter experience is described by analyzing the ache from a mass of other experience and is usually not very satisfactory to the *O*. The first experience is extreme cold, the ache arising rather rapidly and passing, again rather rapidly, into shooting pain. Protocols seem necessary:

"At first it is just cold. The ache comes quickly and is a pressure that surrounds fingers and hands. It is light and pressing, a pressure all over. That is the ache. It also pricks all over; but that is not the ache. The ache was at first in my hand, but became big and bright and went clear up to my shoulder. Attention-demanding but not diffuse."

"The ache is intensive and bright and is inside like a pressure on the bone."

"The ache seems to come from the inside and sends sharp shooting pains up my arm. It developed rather quickly and is bright and big, not very diffuse."

With the first method the ache is produced more gradually. It begins with a pressure which must be abstracted from; but this is never quite satisfactorily done while the experience is still ache. The transition to ache is gradual and at first it is large and diffuse with a pattern of pressure around it and is itself pressury in nature. As the intensity increases a core of experience tends to form which glows 'like warmth.' This experience is dull, diffuse but not as much so as pressure. It is localized. As the pressure continues to increase, the volumic aspect decreases, the margin of pressures tends to disappear, the core becomes more pronounced, brighter, more compact, better defined, and 'more intense' until it becomes 'pain' when it does not appear patterned like the pain of the tooth, before cited, but is still

more volumic than such a pain, not as dense, 'seems different' but approaches such a pain. The brightness of ache is identified with other brightnesses of these experiences.

*Cold.* We mapped an area of 200 sq. mm. on the arms of several *Os*. These were explored four times each; once with the dull point of a pencil, once with a metal point at room temperature (21°C), once with a metal point at about 0°C, and once with a metal point at about −10°C. The mapping with pencil was repeated in some instances three or four times. Instructions called for reports of all experiences, not only the cold. We also used punctiform stimulation on the eye lid. In regard to cold the reports run as follows.

"It is bright and cold. I feel the bright point distinctly and it is cold. I don't know just what the cold is but it is distinct though I can't feel it in the same sense that I do the bright points."

"It is cold and bright. There is nothing there but a bright point but it is cold."

"I don't know just what the cold is or what brings out the report. The obvious experience is a bright point but the bright point is cold."

It is qualitatively the same brightness as in the other experiences reported as bright, *i.e.* contact, prick, pain, etc. Upon arousal the experience is said to lag, that is, the pressure of the point is always felt first, the cold then comes. When the *O* is instructed to report the whole experience there is always, after the first few trials, a pressure component included and this is true for radiant cold as well. The cold 'flashes out,' 'twinkles,' is attention-demanding at first, is compact but hardly dense like prick, well-defined, localized.

Stimulation of the eyelid increases the number of reports of cold, though the experiences continue to be bright. With the stimulation on the hand or arm there is less insistence upon the cold aspect but it is practically never omitted. In a later series with *Os* familiar with the general nature of the problem and instructed to separate the basis of the report of cold from the brightness, none were able to do so and the reports were generally the same as before.

In the mapping of the cold spots it is noticed that if the *O* be instructed to report all his experiences, rather than only to report cold, he many times reports bright experiences other than cold, such as contact, prick and experiences generally described as bright but neither prick nor contact and for these he has no name. This with a cold stimulus, without pricking the skin or more than barely forming contact with a blunt point. There are also many reports of pressure.

It is also noticeable that if one runs over such areas at different times with intensively different stimuli, as described, or if one runs over several times, at different sittings, with stimuli of the same temperature, there is some degree of consistency in the reports of cold but there are many more inconsistencies.

The outdoor cold of a winter day is reported to give rise to a cold and bright experience. But with increase in intensity of the cold or in the time of exposure, areas appear which are pricky or stingy in quality and which do not feel cold.

Our previous report of cold produced by the dentist's drill must be mentioned here as being produced without physical cold as is also the cold of goose-flesh, the cold of fear, the cold shiver produced by scratching on glass and similar phenomena. We endeavored to find a means of producing cold without a cold stimulus which would be available for experimental purposes but were unable to do so.

We wish also to cite the findings of Alston in his work on heat[12] in which he reports that "observer B sometimes noted a pressure component in both heat and cold."

Using as stimulus a solid piece of metal, 6 cm. by 2 cm. by 2 cm., applied to the arm, we get a different type of report. With a stimulus slightly lower than skin temperature such experiences are reported to be 'cool' and are described as all of one pattern or 'massive,' 'flat,' 'dull and diffuse.' They are reported to differ from warm but our *Os* could not describe the difference more than to say 'it is brighter.' They stated the experience to be much more like that of pressure or warmth than like punctiform cold. As the temperature is lowered[13] the experience tends to become patterned: a core of bright and dull experiences within a cloud of dull, pressury experience.

The brightness of the core tends to discrete points of prick, sting and cold and, as the temperature is still further lowered, the core brightens, contracts, becomes more dense. The experience often goes to ache, which has a similar pattern but is brighter, the core more defined, less voluminous and the peripheral pressures are less clear. The ache, at times, but not often under these conditions, goes to pain, in which the same tendencies to change in the pattern are still further carried out. If the experience does not go to ache,

[12] J. H. Alston, The spatial condition of the fusion of warmth and cold in heat, *Amer. J. Psychol.*, 31, 1920, 312.

[13] Temperature was regulated only by a bath. The stimulus was removed, wiped dry and applied. The steps were 5°C apart and went as low as −10°C.

it may go to what is described as a 'painful cold,' a 'cutting cold,' etc. In this the pattern is not advanced as it is when the experience of ache occurs, but there is a pattern of cold where the bright experiences are discrete; these brightnesses, primarily prick and sting but also cold, become intensified within the core.

These experiences are reported to be lively, but not as lively as heat. They are attention-demanding and, when they pass into ache or cutting cold, are reaction-demanding.

*Warmth.* We mapped out an area as before and explored it for warm spots. We did not find well-defined warm spots but we found several areas where the stimulus aroused an experience of warmth. It is 'duller,'[14] or dull like pressure, etc., does not flash out as cold experiences do but wells up and, when distinguished, seems to have already been present for a moment. It glows,[15] is massive, flat, diffuse, voluminous, is not attention-demanding, is localized and varies in intensity. The description of variation of intensity is like that for prick, it is brighter, less massive, more compact, etc. The experience of warmth, so far as our observations go, never occurs without an element of pressure which can be analyzed.[16] Dimmick also found this with aerial stimuli.[17] One of his *O*s described the pressure as soft and mushy. Our *O*s described it, usually, as very soft and faint.

For aerial stimulation we attempted to use an electrically heated rod but found a small electric cigar lighter much more effective and convenient. The reports were practically the same as before when the experience was reported as warmth; but we found a very few cases where a pressure was reported without warmth.

Stimulation with shreds of cotton wool may bring reports of warmth. Two of our *O*s consistently gave such reports. Wunderli[18] found that *O*s might confuse the pressure of cotton and radiant warmth both ways if one placed a paper with a hole in it over certain

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[14]It should be remarked that in the description of the 'dull' experiences, *O*s are much more inclined to use the comparative form 'duller.' The same tendency is shown in other places. Cf. Titchener, *Text-Book of Psychology*, 150; also the article referred to in footnote 6.

[15]The 'glow of warmth' is a much-used expression. We take it to mean a degree of brightness.

[16]Cf. Titchener, *Experimental Psychology, Qual., Inst. Manual*, 1901, 84-85.

[17]F. L. Dimmick, On the localization of pure warmth sensations, *Amer. J. Psychol.*, 26, 1915, 145.

[18]Wunderli, *Experimentelle Beiträge zur Physiologie der Tastsinnes* (Diss.), 1860; *Molesch. Untersuch.*, 7, 1860, 393. We used O. Funke, in Hermann's *Handbuch der Physiol.*, 3, 1880, ii, 321-324.

parts of the body and then stimulated through the hole. Under conditions similar to his our *Os* reported a warmth component with the cotton stimulus but also a pressure component, and with the radiant warmth they reported both. They were not instructed to guess at the objects but the patterns reported differed for the two experiences.

Observer M, in the work on heat by Burnett and Dallenbach,[19] speaks of "a pressure that at first appeared to be warm."

For the following experiment we used the metal piece described above. This was heated in a bath the temperature of which was our guide as to the temperature of the metal piece when applied; although it was necessary to dry the piece before using it. We used temperatures in steps of five degrees up to 60°C.

The description of warmth does not vary materially from that given above except that, as the stimulus becomes warmer, the resulting experiences of warmth are reported to be brighter, better defined, less voluminous, etc.

At about 45°C (not exactly determined) discrete bright experiences, prick, sting, sometimes cold, begin to occur. As the temperature of the stimulus rises we get experiences of heat which, as patterns, do not differ materially from the patterns reported in other studies[20] except that the experiences become increasingly lively, the brightness of the core becomes more dominant in the experience and the cloud of warmth which surrounds the core tends to fade.

The pattern, as described, resembles that given for cold and the similarity was noticed by some of the *Os*. It differs in that the experience of heat is livelier, more threatening and the peripheral cloud in heat is usually described as warmth whereas that for cold is usually described as pressure.

*Heat.* The following discussion of the results of other investigators of the experience of heat is presented here in lieu of observations of our own and is given for the purpose of showing heat to be a complex and patterned experience rather than a simple and unanalyzable quality.

The description of the experience of heat began with Alrutz[21] who reported that upon stimulating warm and cold spots simultaneously with aerial stimuli he

[19]N. C. Burnett and K. M. Dallenbach, The experience of heat, *Amer. J. Psychol.*, 38, 1927, 422.

[20]See references in next section on 'heat.'

[21]S. Alrutz, *Upsala läkaref. förhandl.*, 2, 1897, 340-359; *Mind*, 7, 1898, 141-144; *Skand. Arch. f. Physiol.*, 10, 1900, 340-352. (We used *Mind* only.)

experienced another sensation which was neither warmth nor cold but which he proposed to call 'heat' (Hitzempfindung). This experience, he maintained, was *generally*[22] simple and unanalyzable. He does not state how he analyzed it when it was analyzable so we may infer that he analyzed it into warmth and cold, or believed that he did. He would hardly have omitted to mention the fact if he had analyzed it into anything else.

He accepted the work of Thunberg as supplementary to his own although Thunberg did not accept the idea of its being unanalyzable.[23] He states that with practice it may be analyzed and in this Kiesow, who also worked with heat, agrees.[24] Kiesow goes further. He believes that there may be other sensations involved than warm and cold, that there may be all sorts and degrees of fusion and he notices that cold and pain are of especial significance for the qualitative coloring of these fusions.

Cutolo[25] concludes from his study that heat is a simple quality but he does this by disregarding the evidence of two *Os* (he used only three) who persisted in partly analyzing the complex, and accepting the reports of the third *O* because he was more practiced.[26] One *O* (K) analyzed the experience as 'warmth with a slight pain element' twenty-nine times without once naming it as a unified experience. For the second *O* (G) the report of the problem states that "frequently two qualities were reported as appearing simultaneously."

The third *O* (Boring) reported 'heat' without analyzing it in the sample report offered. It was, of course, within a pattern which contained other experiences, but was experienced as heat. This *O* is quoted as "referring to heat as an unique but not necessarily simple experience, the main characteristic of which was a quality something like pressure and something like pain." Later he calls this quality 'sting' and describes it further as "flat like pressure and bright like pain." But he finally concludes that " 'true heat' is a simple pressure-pain quality, with no trace of warmth or cold in it, and that warm-heat and cold-heat are fusions." Just what 'true heat' implies is difficult to conjecture, but it may be a heat experience that is perfectly fused, or one that is perfectly analyzed out of the complex or it may be a heat that is attentively regarded as a unit, which, under another set (for analysis) would have been broken up.

Cutolo concluded for the simple nature of the experience; but in the next problem reported, as we find, that the *O* in question expresses a doubt about the attentive factor and does analyze heat more than once.

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[22]The italics are his.

[23]T. Thunberg, *Upsala läkaref. förhandl.*, F., 1, 1896, 489 ff. Cf. also Nagel's *Handbuch der Physiologie des Menschen*, 3, 1905, 707.

[24]F. Kiesow, Zur Analyse der Temperaturempfindung, *Zsch. f. Psychol.*, 26, 1901, 231-246.

[25]F. Cutolo, Jr., A preliminary study of the psychology of heat, *Amer. J. Psychol.*, 29, 1918, 442-448.

[26]This is not meant as an unfavorable criticism. The experimenter probably knew the degree of training of his *Os*. Yet it is a fact and must be mentioned when we go back of Cutolo's conclusions to the evidence of his *Os* or that part of it which was published. Boring, the *O* in question, had undoubtedly had much more training at this time than either of the other *Os*. It is more important to us as he is the only person who appears as an *O* in two reported studies on heat.

Alston was particularly interested in the space conditions for the fusion, but he gives some qualitative reports.[27] Here we again have three *O*s who describe heat. B (Boring) states, toward the beginning of the work, that "normally heat is a simple pressure-sting"—which is substantially what he reported before. Y laid especial emphasis upon the brightness of heat, and B (in some reports) agrees with him. P calls it 'sharp' qualitatively. We might argue that P, being musical, used the term sharp instead of bright, but it does not matter. We may conclude that all three *O*s noted (analyzed) the quality of brightness (or sharpness) in the experience.

Alston finds that the experience rather easily breaks up into warmth and cold and he suggests either a physiological or an attentive explanation.

During the course of this work we find B's attitude gradually changing. Nine times he reports a psychological fusion of warmth and cold in heat. "A faint warmth and a faint cold were well fused, but not perfectly fused, into heat." "Heat, warmth and cold are simultaneous, but I can't attend to the heat and to the warm-cold at once." The author states that "B has a great deal to say of the difficulty of attending to cold-warmth and to heat simultaneously when the possibility of attention to all is present. He seems to take this situation as indicative of fusion." Again: "B sometimes noted a pressure-like component in both heat and cold." All the *O*s agreed that there was some kind of a qualitative similarity between heat and cold. Heat is more often referred to the cold than to the warm spot. Volumically heat is found to be sometimes small and well-defined and sometimes large and diffuse.

Burnett and Dallenbach are the joint authors of the most recent experimental work done on the subject.[28] Their summary of results shows that they have concluded from their facts that Cutolo was justified in his conclusions and that "heat is psychologically a simple quality that lies in the pressure-prick-pain continuum, near prick and closer to pressure than to pain."

Again we must go to some of the individual reports which are offered for evidence. D (Dallenbach) says: "the heat stood alone. . . [It] became more and more intense. . . . The quality of the heat was at first pricky, drawy, tingling. *These qualities* became more and more intense as the intensity of the heat advanced. At the end of the experiment a new quality of smart was *added* to the heat." This is the language of analysis of components, not the description of a simple quality.

*O* H is quoted as reporting "A mild hot: tingle, penetrating *components*[29] and bright pressures constitute the body of the experience." Again: "The matrix of warmth refers to the fact that behind the bright, stingy, pressury quality there was a warmth, glowing and solid in nature." He frankly speaks of components and these are continued throughout his observations.

The facts in regard to the simple nature of heat would seem to be as follows. Alrutz analyzed heat a part of the time. Thunberg and

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[27] J. H. Alston, The spatial condition of the fusion of warmth and cold in heat, *Amer. J. Psychol.*, 31, 1920, 303-312.

[28] N. C. Burnett and K. M. Dallenbach, The experience of heat, *Amer. J. Psychol.*, 38, 1927, 418-431.

[29] All the italics are mine.

Kiesow did so regularly. Two of Cutolo's three *O*s analyzed it and the third did so eventually. All three of Alston's *O*s analyzed heat, at least analyzed the bright aspect out. One *O* analyzed it often, even into warmth and cold, and declared the fusion 'not perfect.' 'Not perfectly fused' can only mean 'not a simple quality.' In the last work at least two of the *O*s were analyzing heat a part of the time; although they believed that the experience, at other times, could not be analyzed. That was exactly Alrutz's and Boring's difficulty. It is easily explained on the basis of attention, an explanation offered by Alston and certainly suspected by Boring. But that point is not essential here. It is sufficient for us that the experience is sometimes analyzed, and if so it is not simple. The contrary seems to be implied by some of the authors whose work we are considering. But this is impossible. No one maintains that all the notes of all musical chords are analyzed. They are ordinarily fused, at least attensively, into unitary patterns. The test of their complex nature is whether they can be analyzed at all—when one has a set to analyze.

Such experiences as intermediate color qualities and the fusion of two intensities in audition are said to be 'unanalyzable.' But this means that even with a set for analysis no one can, at any time, analyze them into their components. Our parallel case, in this instance, certainly is that of the tones; and the fact that heat (and other felt experiences as well) is usually accepted as unitary does not prove it to be simple and unanalyzable.

We may safely conclude from the evidence presented in the studies cited, as well as from our own observations, that the simultaneous application of warm and cold stimuli on adjacent areas or the application of a single stimulus of 45°C or over, produces a complex experience consisting of both bright and dull elements. The pattern consists of a core of lively bright elements partly fused with duller components, and a cloud of warmth which envelopes the core. The bright components may include sting, prick, weak pain, and cold; and the duller components may include pressure and warmth. This experience is called 'heat.'

If we abstract the core from such an experience we find that it alone is also patterned as described above but lacks the cloud of warmth.

When not analyzed but regarded as a unit the brighter components dominate attensively and it is reported as a bright, lively, threatening (reaction-demanding) and attention-demanding experience.

## Discussion of the Facts

We have offered clear evidence of the complex and patterned nature of these experiences. We have gone at least part way in the analysis of all of them. With pressure, warmth and cold, the more nearly perfect fusion of components, and with pain the attentive dominance of one component, have made the analysis more difficult. The relative simplicity of these experiences is evidenced by the fact that many of the grosser patterns are described in terms of the simpler. But all have yielded, in some degree, to analysis.

It must be remembered, in this connection, that there is no experimental evidence offered for the absolute simplicity of these experiences, except heat. Their unanalyzability has been generally assumed but only empirically determined, and the slightest showing of fact must be sufficient to create a presumption in favor of their complexity.

The aspect of brightness or sharpness[30] is reported throughout as the only qualitative aspect of felt experiences; but this brightness, if it is the qualitative aspect of these experiences, varies as pitch or brightness in audition or as brightness in vision rather than as the colors in vision. But even if we accept this, there is obviously some confusion with intensity which must be cleared up before we can be reasonably sure of our terms.

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[30]These terms also may prove difficult for those unaccustomed to their use. It has been maintained that pain is a simple quality and that prick is the same quality in less degree. The idea was defeated by calling attention to the fact that prick is not painful, no matter how much it may suggest pain. The further fact that we have different pains of equal intensity, *e.g.* pain of tooth and pain of ear, as described, which are differentiated by every observer, shows that there is more than one quality of pain. The aspect of these experiences which is simple and identical for most observers is the brightness or sharpness, a fact that any one may easily demonstrate to his own satisfaction. If a feeling of prick should become more intensive or, as usually expressed, brighter or sharper, it might become pain, just what kind of a pain it became would depend upon how and where the stimulus was applied, in other words, upon the whole pattern set up. This same aspect of brightness has, previous to this study, been found in other experiences, notably in contact and cold, and references to these facts are frequent in the literature.

The use of the terms brightness and sharpness, and their complements, dull and flat, has been current in the Clark laboratory for some years and the reports quoted show that it is in use in some other laboratories, so it need not be surprising that our *Os* use the terms freely.

That these expressions are also used in audition and vision is an interesting fact. Sharp and dull or sharp and flat are used in all sense departments. All of these terms are probably primarily visual as are also the terms used to express volume, acuity and density. Such a common usage may indicate nothing more than a poverty of language; but it may, and we venture to assert that it probably does, indicate something common to all of these experiences.

So far as our reports go, the separately mentioned aspects of these experiences are not shown to be independently variable. Flat and sharp or penetrating seem to be used just as the words dull and bright are, that is, they are identical aspects. Apparently volume varies inversely and density, clearness, and definiteness of outline vary directly as the degree of brightness varies. But these aspects are not definitely shown to be dependent.

All felt experience seems to be localized, how accurately we did not attempt to determine. There is already much in the literature on the localization of some types of these experiences.

## The Analogy with Audition

The similarities between the facts of this department of sense and that of audition are very noticeable. We have several times made use of these analogies in the body of this study and the following are offered in the belief that they may possibly prove to be more than a mere analogy.

It has often been remarked that auditory experiences terminate, at the lower end, in a pressure-like, dull, flat quality, and at the upper end, in distinctly painful bright tones. This in itself is a relationship between the senses and it has led us, for years, to look for some other relationships between the two.

It now appears that each of these modalities has only one qualitative aspect and for this the same names, flat-sharp or dull-bright are chosen.[31] The use of the words sharp and flat in music is particularly enlightening for us here as the relative nature of the difference is apparent—any tone is sharp as compared with any other if it is 'higher' in the scale and is flat if 'lower.' (The terms 'high' and 'low' are, of course, also primarily visual.)

They are again similar in the way in which these brightnesses form patterns and, at the same time, maintain their identity. In audition partials may be 'heard out' of tones as separate, less intensive tones just as prick, pressure, etc., are 'felt out' of some of the comparatively simple feeling patterns. This resemblance in pattern formation goes still further and into the more complex structures for as tones, which are themselves unit patterns of different bright-

[31]The identity of pitch and brightness in auditory experience is not accepted universally. Cf. R. M. Ogden, *Hearing*, 1924; but also G. J. Rich, A study of tonal attributes, *Amer. J. Psychol.*, 30, 1919, 121-164.

nesses, are heard out of a chord, so a group of pressures were analyzed out of 'roughness' by Meenes and Zigler[32] and warmth and pressure were analyzed out of the experience of 'oiliness' by Sullivan and Cobbey.[33]

Both types of experience are localized and in both we find the aspect of volume, more obvious, perhaps, in feeling than in audition but certainly reported in both. There are reports on intensity in both and the ear is evidently partly an organ of hearing and partly an organ of feeling.

The type of analysis given by persons without psychological training shows the similarity in structure between the two types of experience. Such a description as 'middle c on the piano' is parallel to 'smart of an open wound,' *i.e.* both the *quality* and the *guide for timbre* must be specified.

The specific patterns are much alike. In audition the brighter experiences are described as sharp, well-defined and dense, and just those words are used to describe the bright felt experiences. Dull means, for both, flat, diffuse and volumic. The analogy cannot, at present, be completed for there is evidence to show that volume, in auditory experience, is an independent variable while that remains to be shown in feeling and, in the latter modality, intensity is confused with other aspects while in audition it is apparent as a separate variable.

## The Touch Pyramid

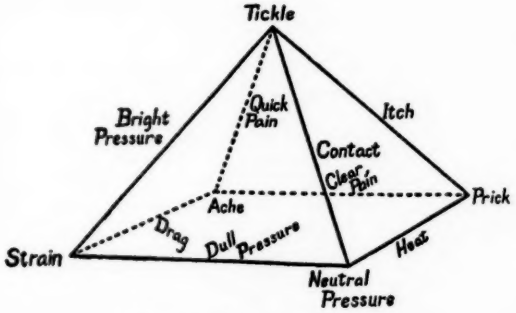
We present a copy of the 'touch pyramid' as formulated by Titchener.[34] It is the result of his effort to find resemblances between the qualities enumerated above and to give them a logical place in a system such as the qualities of vision find in the color pyramid or color cone. He was not entirely satisfied with the figure but sufficiently so to present it to the public.

One difficulty, as he states, is that the qualities of warmth and cold are not represented, whereas they should find a place in any such schema. But there are other difficulties. Tickle, contact, and neutral pressure he believed to be, stimulus-wise an intensive series[35] but it has been shown that tickle is correlated with double stimulation. Nor is it a qualitative series, for tickle and contact have been shown to be identical in quality,[36] and no one has ever shown that con-

[32] M. J. Zigler and M. Meenes, *ibid.*, 34, 1923, 542-549.
[33] A. H. Sullivan and L. W. Cobbey, *ibid.*, 33, 1922, 121-127.
[34] *Amer. J. Psychol.*, 31, 1920, 213.
[35] Here we draw upon lectures which have not been published.
[36] See references cited in footnotes 7 and 8, above.

tact and neutral pressure closely resemble one another. The most casual observation will show that they do not. These facts defeat the idea of its being either an intensive series stimulus-wise or a series of psychological qualities.

The series, tickle, itch, prick will not do as a qualitative series for similar reasons; but, as a matter of fact, Titchener did not mention these together but rather: itch, prick, pain, which he regarded psychologically as an intensive series. Even if we admit this (which, without reservations, we cannot do) it prohibits its also being a qualitative series unless a condition can be shown in this sense department similar to that in vision. Otherwise the fact that they are intensively different would lead one to conclude that they are, for him, qualitatively the same; otherwise why group them? Titchener himself has stated that prick and pain are qualitatively identical.




TITCHENER'S TOUCH PYRAMID

The rest of the qualities on the pyramid are chosen for their simplicity and placed in the best relative positions possible under the circumstances—except heat. This is the one experience which seemed to fulfill all the conditions for an intermediate in a qualitative series as it was reported to be 'like pressure' and 'like prick or pain' and at the same time was reported to be a simple, unanalyzable, quality. The reassurance that came with this description probably saved the pyramid for many a day. From our own reports and from other published studies we have shown the complex nature of heat and, incidentally, of the other two members of the series; with the failure of this series the pyramid loses its last and only real support.

It is not in these difficulties of detail, however, that we wish to show the inadequacy of the schema for its purpose; they were, for the greater part, realized by its author. More generally we believe that we have shown that because of the complex nature of these experiences they have no place, and cannot be made to fit, in any schema representing the qualities of felt experience.

## Affective and Emotional Experiences as Sensory Feelings

In 1908 Titchener published his *Psychology of Feeling and Attention*.[37] As sensation was to perception, and as image was to idea, so the feelings (affective qualities)[38] were to emotion; and these two, feeling and emotion, were in a category by themselves, as a kind of experience different from all other kinds. To shut this veil of mystery down tight on the facts of the matter, the criterion of the affective experience was its unobservability. This in an *existential* system! By 1921 he had realized the impossibility of such a condition of affairs without much urge from the outside world. The result was the setting of the problem on the affective qualities which was published in 1924 (footnote 6) and which reduced pleasantness and unpleasantness to sensory experience.

The affective terms are also (legitimately enough) used in other ways. To say a thing is pleasant may only mean that it is desirable, that one would like more of it, or even that it is not unpleasant. But there is such a thing as a felt experience which we also call pleasantness, which, with a corresponding unpleasantness, was the subject of that dissertation. Pleasantness and unpleasantness were found not to occur alone but to make up a part of unitary patterned experiences. Qualitatively regarded the experience added in the case of pleasantness is bright-pressure and in the case of unpleasantness, is dull-pressure.[39]

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[37] *Lectures on the Elementary Psychology of Feeling and Attention*, 1908.

[38] Titchener uses the word 'feeling' as equivalent to 'affective qualities,' never to cover sensory experience, as the word is used in this study. This usage got him into a linguistic difficulty, for it forced him to choose another word as a general term for sensory experience. He chose 'touch' but it is not generally understood in this sense and 'doesn't sound right.' So, when he was forced to find still another general term, he had to choose 'pressure,' hence 'bright-pressure,' 'dull-pressure' and the likes.

[39] These terms are described above (cf. footnotes 6, 30, and 38). The fact that the *O*s in that problem used the terms easily when, as it appeared later, they were unfamiliar to most psychologists, gave the whole problem an air of unreasonableness. Especially as one of the *O*s was a sophomore. But it must be remembered that this sophomore had had the beginning course in psychology at Cornell and she was familiar with the terms and what they meant within the year she acted as *O*. Familiar but not practiced. On one occasion she used the expression 'bright-feeling' and in the same paragraph corrected herself to 'bright-pressure' (p. 521). The other *O*s had used the expressions longer and did not happen to fall into such an error. Even at that, several *O*s specified (*e.g.* H, p. 518): "It's like bright-pressure with the pressure left out." Some *O*s occasionally reported a pressure element, *e.g.* "it is soft like warmth;" but the word 'pressure' was usually used to mean only a 'felt' experience. Any pressure found was reported for itself. The confusion is very bad, the more reason for ending it. Cf. also footnote 38.

This problem restored the affective qualities as observable phenomena and furthermore put them in the same category with other experience (sensory); but they were found to occur only as undivided parts of other experiences, never alone. Experience always comes patterned and when an experience becomes an affective experience the pattern changes. For our present purposes there are enough and to spare of descriptions of such patterns in the original article.

But this carries us a step further. If these affective experiences are *patterns* of sensory experience, then the function of the affection to form a core for emotional experiences has disappeared, and if these experiences are not 'simple,' in a Titchenerian sense, by what criterion shall we distinguish an affective experience from an emotional one? There appears to be none. Psychologically an emotion and an affection must be defined alike and both are complexes of sensory experiences. The terms 'emotion' and 'affection' may still be useful words to express degrees of complexity, particular types of pattern, methods of arousal, or to differentiate one type of felt experience from another.

The older distinctions had their being, from the very first, only for systematic purposes. There never existed any considerable evidence for them and common sense and the science have never really given up the use of the word 'feeling' to include the whole gamut: touch, kinaesthesis, organics, affective qualities and emotions; nor has it abandoned the word 'touch' for cutaneous sensitivity and 'pressure' for a certain general pattern of felt experience, all with variations.

## Some Theoretical Implications

It is impossible, in such an article, to treat theory with any degree of thoroughness. There are certain obvious implications within the study which may be discussed with profit; but the facts are still meager and the great amount of valuable work done under present theories, regardless of the fact that the theories themselves have not advanced, necessitates a thorough and careful consideration of the whole subject, which promises to be especially voluminous since we must include the work on affection and emotion.

The results of this problem have not complicated the general problem to any great extent for the aspects reported upon have shown little evidence of independent variability. Yet, slight as the evidence is, we suspect that both volume and intensity will eventually prove

to be separable from quality. It would be useless to begin to look for a theory of any sort until such facts are determined. Localization must be accounted for also but, under all the circumstances, one anticipates less difficulty from that source.

It cannot be successfully maintained, in this instance, that economy calls for a theory of a single nerve supply because we have found a single quality. The facts found in other studies make such an assumption highly improbable.

The discovery of the experiences of warmth and cold as complexes of small units of patterned brightness prompts us to cite the following considerations in regard to these two qualities which, it would seem, must be included in any theory attempting to account for them.

*Cold.* Our *Os* found, in punctiform experiences of cold, brightness and slight pressure. We discounted the pressure at first as an artifact due to the nature of the stimulus, as many others have done, until we found that with aerial cold the pressure element still remained.

With aerial cold the experience is more gross but the idea, which seems to be harbored by some, that a punctiform stimulation arouses impulses over a single nerve, is absurd. The Krause end-bulbs, and the other modified nerve endings as well, are known to have more than a single nerve supply, and the chance that we consistently stimulate even a single ending is extremely remote. The simplicity of the experience aroused with punctiform stimulation is only a *relative* simplicity—it stimulates a smaller area than the aerial stimulation does and that is all. In the more gross experience the previous 'compact, well-defined, bright *spot* with a pressure element' becomes a 'compact, well-defined, bright *core* with a cloud of pressure around and through it.' This is a similar pattern on a larger scale and the theory that its arousal is dependent only upon the nature of the physical stimulus must face the following facts, as shown by our study, by the work of others and by common knowledge.

(1) An experience of cold may be aroused without a physically cold stimulus: (*a*) in fear, pain, goose-flesh, scratching on glass, etc.; (*b*) by the application of a very warm stimulus (over 45°C.). This is Von Frey's 'paradoxical cold' and it is to be noted that he reports that this occurs, not at the predetermined 'cold-spot,' but around it.

(2) A physically cold stimulus, when applied, may fail to arouse an experience of cold. In this we refer to the fact that frequently, in searching mapped areas, spots reported to feel cold at one trial fail to produce the experience at another time when every precaution is taken to stimulate exactly the same spot in exactly the same way with the same stimulus at the same temperature, adaptation or fatigue being avoided. There is the added fact that very few points on the skin, if any, are always sensitive to a punctiform cold stimulus.

(3) An experienced cold changes to an experience of prick and sting when the physical stimulus remains the same, only the time of exposure being increased, *e.g.* exposure to the air on a cold day.

(4) An experience of cold set up by a physically cold stimulus is modified to include other bright experiences, prick and sting for example, if a warm stimulus is introduced upon an adjoining area, *e.g.* the heat grill.

These facts, although we do not offer them at this time as conclusive, indicate strongly that a certain physiological, or other, pattern *in the skin* is accompanied by an experience of cold. That this pattern is *usually* aroused by a physically cold stimulus but the experience may be aroused without it and, even when present, it does not always prove effective.

*Warmth.* The facts for the experience of warmth are similar and, so far as general theory is concerned, lead us in the same direction. The experience consists of a diffuse glow of brightness with a pressure element found by Dimmick, Wunderli, ourselves, and others.

The experience, again, may be set up without a physically warm stimulus (Wunderli's and our reports) and a punctiform warm stimulus[40] does not always set up the experience. Such an experience, when aroused, and when the physical stimulus remains unchanged, is modified by the presence of a cold experience on an adjacent area.

## Conclusions

From the results of this study we conclude:

(1) That the 'qualities' of felt experiences, as generally recognized, are not simple and unanalyzable, but are analyzable patterns of experience.

(2) That Titchener's 'touch pyramid' is inadequate to the facts and that any attempt to show qualitative series, using such experiences as examples of simple qualities, cannot logically be accomplished.

(3) That felt experiences are, qualitatively regarded, patterns of brightness (or sharpness) of greater or lesser complexity.

(4) That, in structure, such patterns resemble tones (timbre).

(5) That such experiences vary in brightness, volume, density, definiteness of outline, and may vary in intensity, but such variation is not shown, in any case, to be independent of variation in other aspects.

(6) That all such experiences may be localized.

(7) That affective and emotional experiences are fundamentally similar to other ('sensory') felt experiences.

(8) That the proper name for this modality, including cutaneous, kinaesthetic, organic, affective, and emotional experience, is the sense of 'feeling.'

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[40]The cases which we reported with aerial stimuli were altogether too few to offer as evidence. We mention them in the body of our report to encourage others to attempt to find similar cases.

# THE CONFLICT THEORY OF EMOTION

By Roswell P. Angier, Yale University

In 1894-95—at about the same time that James[1] was firing his last shot at the critics—John Dewey published two articles on emotional theory.[2] They fell flat. I can find no review, discussion, or even specific mention of them at the time or during the years immediately following, with one minor exception, in the two journals of the country mainly devoted to psychology—*The American Journal of Psychology* and the *Psychological Review*. Nor, with an exception or two, have Dewey's ideas apparently influenced subsequent thought about emotion. A fair indication of the general excitement aroused by any theory is, I imagine, the extent to which it directly prompts citation or discussion in our textbooks, or more indirectly colors their exposition. I have looked through some dozen or two of the psychology texts that have appeared since the beginning of the century, and only one of them specifically concerns itself with Dewey's ideas;[3] the others show almost no trace of even indirect influence, with one exception.[4] Angell[5] wrote that Dewey's "brilliant formulation" suffered by the inability of the public to understand it. This is, I think, the main reason for the neglect of it. Other thinkers have, however, obscured their thought by abstract and perverse style and have yet influenced the course of speculation. Did Dewey's thought itself therefore lack the cogency necessary to secure its persistence in spite of its forbidding verbal form?

I believe his basic conception to be perhaps the most significant—next to James's—that has appeared in emotional theory. It is therefore the purpose of this article to make a brief and modest attempt at an *Ehrenrettung* of Dewey's main contentions, preliminary to a later more detailed discussion of emotion elsewhere, and to add per-

[1]William James, The physical basis of emotion, *Psychol. Rev.*, 1, 1894, 516-529.

[2]John Dewey, The theory of emotion: I. Emotional attitudes, *Psychol. Rev.*, 1, 1894, 553-569; II. The significance of emotions, *ibid.*, 2, 1895, 13-32.

[3]J. R. Angell, *Psychology*, 4th ed., 1910.

[4]B. B. Breese, *Psychology*, 1917.

[5]J. R. Angell, A reconsideration of James's theory of emotion in the light of recent criticisms, *Psychol. Rei* , 23, 1916, 251-261.

haps a few remarks that will add plausibility to the concept of 'conflict' as the constitutive mechanism in emotion. It is particularly fitting that something of this nature be ventured at the present time, since the psychologist in whose honor this volume is issued began her teaching not long before Dewey's articles appeared, and since Dr. Dewey himself is shortly to be honored by a group of friends and colleagues.

Dewey says at the start, simply enough, "I propose, assuming Darwin's principles as to the explanation of emotional attitudes, and the James-Lange theory of the nature of emotion, to bring these two into some organic connection with each other, indicating the modifications of statement demanded by such connection."[6] He then at once partially reformulates Darwin: the so-called 'expressions' of emotions are survivals, not of acts once useful in giving effect to *emotions*, but of acts useful to the organism *qua acts;* they can be regarded as expressions only when looked at "from the standpoint of an observer." Primordially they were merely serviceable responses to environmental situations, their *raison d'être* lying in their adaptive utility; they survived in the evolutionary series as *attitudes*—the residual reactive patterns normally left by repetitions of immediately adaptive instinctive or habitual acts. The reference to emotion in accounting for the genesis of the attitude is quite irrelevant. Confronted by a situation to which it is wholly pertinent the attitude goes over at once into adaptive activity; confronted by a situation to which it is not completely pertinent it is held in suspense and stands out *qua* attitude.

To be explicit, situations arise (in Darwinian parlance, later on in the evolutionary series) partly identical with those originally conditioning the attitude, which rearouse the attitude; but while some features of the situation evoke appropriate reactions, others do not; the total attitude cannot, therefore, go over without remainder into acts that are useful but is at least partially aborted. There is, consequently, tension, or conflict, within the attitude or, if one will, conflict among the diverse reactions incipiently or overtly issuing from it. In grief, for instance, reactions surge forth from an attitude rearoused by some phase of a situation (the room of a friend who has died), but the really significant customary feature in the situation is gone and the resultant reactions interfere with one another. This inevitable

[6]Dewey, *op. cit.*, I, 553.

tension, interference, mutual checking, friction, struggle, competition, or conflict (terms used synonymously by Dewey) between the pertinent and non-pertinent or aborted features of the total response is the immediate condition of the "emotional seizure." If this be so, then uninhibited activity, however vigorous, is unemotional. "The familiar fact," says Dewey, "that emotion as excitement disappears with definiteness of habit simply means that in so far as one activity serves *simply* as means, or cue, to another and gives way at once to it, there is no basis for conflict and for inhibition. . . . The activity of seizing prey or attacking an enemy, a movement having its meaning in itself, is now (*i.e.* in the emotional situation) reduced or *aborted;* it is an attitude simply. . . . There is no reason to suppose that the original activity or seizure was emotional, or had any *quale* attached to it such as we now term 'anger.' The animal or our ancestor so far as it was given up without restraint to the full activity undoubtedly had a feeling of activity; but just because the activity was undivided, it was not 'emotion.'"[7]

Now "if every emotional attitude is referred to useful acts" then the emotion cannot be "the antecedent of the attitude." It slipped in as prior, with the acts as consequent "expressions"—as already indicated— only in the reflection of the observer. The emotional seizure must, then, itself be the consequent of the acts and attitude, and in some way "constituted by the reflexion of the teleological attitude, the motor and organic discharges, into consciousness. . . . The fact, if it be a fact, that all 'emotional expression' is teleologically determined, and not a result of preëxistent emotion, is itself a strong argument for the discharge (James's) theory."[8]

Dewey's further task is now to buttress his general argument and also to exhibit the psychological mechanism by which conflict becomes the basic condition of emotion. He distinguishes at once between a *total act* loosely termed "emotional" and the harbored core of *Affect*, which is the emotional seizure proper, merely lending its name to the act in its entirety. Any such total act thus labeled is really but "a certain disposition, a mode of conduct, a way of behaving"—a mode of behavior, furthermore, "which is purposive," or has "an intellectual content" or "object,"—and its emotional core is but "the subjective valuation of that which is objectively expressed in the idea or

[7]Dewey, *op. cit.*, II, 26 ff.
[8]*Ibid.*, 13.

purpose."[9] Here lies the nerve of the discussion. How does this emotional core get there, or in Dewey's phraseology, how is one "to *place* this 'feel' with reference to the other phases of the concrete" act; specifically, with reference to the 'idea' or 'object' which, as we shall see, it is also the function of the total act to define? Here he parts company from James.

The relation is certainly not, first, the object or idea operating only as stimulus; secondly, the mode of behavior taken as discharge of this stimulus; and, thirdly, the *affect* as sensory repercussion of the discharge. There is no such splitting up into three separate parts, of what is in reality a total course of behavior proceeding towards a consummatory adjustment in the progress of which, as two part processes, the 'idea' or 'object' and the emotional excitation are "*constituted at one and the same time.* . . . It is sheer reflective interpretation to say that the activity in anger is set up by the object, if by object we mean something consciously apprehended as object. . . . If my bodily changes of beating heart, trembling and running legs, sinking in the stomach, looseness of bowels, etc., follow from and grow out of the conscious recognition, *qua conscious recognition*, of a bear, then I see no way for it but that the bear is already a bear of which we are afraid—our idea must be of the bear as a fearful object."[10]

This refusal to admit disjunction between stimulus, response, and affect is central in Dewey's general psychological slant, elaborated in a later discussion criticizing the conventional reflex-arc concept—to which I shall have to refer the reader for fuller statement.[11] So far as its application to the present problem is concerned the argument would run somewhat as follows: Movement and stimulus are interlocking, altering their nature *pari passu* as the total mode of behavior proceeds, each serving to modify the other in the sense of clarifying or defining (for action or consciousness) just what the other *is*. "It is just as true to say that the sensation of sound (or sight) arises from a motor response as that the running away is a response to the sound. . . . Indeed the movement is only for the sake of determining the stimulus, of fixing what kind of stimulus it is, of interpreting it."[12] Since the exact nature of the "stimulus" (*i.e.* the 'object' or 'idea') is

[9] *Ibid.*, 15.
[10] *Ibid.*, 18 f.
[11] Dewey, The reflex arc concept in psychology, *Psychol. Rev.*, 3, 1896, 357-370.
[12] *Ibid.*, 362 f.

progressively explicated, but not fully determined, until the act has run its course, the stimulus cannot serve initially as a full fledged thing to determine another full fledged thing called an act. "The stimulus must be constituted for the response to occur."[13] The serially occurring acts condition or define the idea or object, just as the idea in its progressive stage towards complete clarity gives significance to the accelerating adaptiveness of the acts.

I have purposely kept for awhile within Dewey's perhaps somewhat estranging conceptual formulation. But it should be clear where he places the emotional "feel" in relation to the object "exciting" the emotion and to the total form of behavior groping for organic adjustment, of which "feel" and "object" are partial and synchronously determined phases. To be concrete, "the 'bear' is constituted by the excitations of eye and coördinated touch centres, just as the 'terror' is by the disturbances of muscular and glandular systems. . . . It is precisely and identically the same actual concrete experience."[14] The bear is not the object bear-to-which-the-feeling-attaches "*except* as the fright comes." . . . "The factors of a coördination (whether due to inherited instinct or to individual habit) begin to operate and we run away; running away, we get the idea of 'running-away-from-bear' or of 'bear-as-thing-to-be-run-from' " (p. 22). It is irrelevant to the main argument whether or not one adopt Dewey's dualistic bias of "objective idea or purpose," on the one hand, and "subjective (emotional) valuation," on the other. What is really relevant may be expressed by any brand of psychology—that emotional seizure is but one constituent part of the process of attaining a total envisagement of, or consummatory adjustment to, the situation that initiated the process. But the seizure, *qua* seizure, is certainly not the adaptive pattern. The latter may be running from a bear, demolishing an opponent's face, or schooling oneself to self-control when emotionally beset. In any case a tripartite disjunction into object (stimulus), act, and seizure (whether conceived as precedent to, or consequent on, the act) is out of place; for all are simply distinguishable developments within a progressing total behavior pattern.

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[13] *Ibid.*, 368.

[14] Dewey, The theory of emotion: II. The significance of emotions, *Psychol. Rev.*, 2, 1895, 20 f.

[15] *Ibid.*, 15.

So much for Dewey's main conception of emotion as an incidental but necessary development, in certain types of situation, within a total form of behavior seeking adjustment to the situation. But the narrower locus of the emotion, as has been stated, is a specific kind of conflict necessarily generated by such behavior. We may now discuss a bit further the *raison d'être* of this conflict. Past useful acts (whether, as Dewey says, due to inherited instinct *or* to acquired habit) have left as residua certain ready 'attitudes' to action. These may be rearoused by present situations only in part identical with the past situations that evoked the useful reactions generative of the attitudes. The present situation, therefore, exhibits features to which some of the acts appropriate to the attitude are not pertinent. Hence, until a further stage of comprehension or reënvisagement of the confronting situation occurs no inclusive and relevant consummatory adjustment to the situation can ensue. But those groping reactions which will result in this adequate comprehension must be adjusted to the non-relevant features of the partially aborted attitude; hence, as an inevitable incident in the reconstructive process, a mutual deflection, or tension, or inhibition or interference, or conflict between the two phases of the total process develops. This inhibitory activity *is* "the disturbance of the emotional seizure."

Consider, in illustration, Dewey's own recurrence to James's bear episode. The initial "act of seeing" or "optical-ocular" act, acquaints the observer with some change in the field of vision but does not at once give any clear 'idea' of what it is; the 'object' is still awaiting definition. The 'seeing' therefore at once instates some habitual attitude, perhaps one of expectant attention—pause, immobility, senses keenly alert. This attitude, being wholly pertinent to this stage of the situation, secures *free and full discharge;* there is no conflict of contrary 'sets' or acts; no emotion. But let some further change in the still unclarified configuration involve the arousal of some equally ingrained but incompatible attitude—perhaps an incipient set towards running away—and the entire action pattern is altered. The observer may still hold his ground; the attitude of expectancy still holds over and continues to operate, along with the evoked tendency to run; the 'object' is not yet clearly 'ideated.' The two active attitudes must be adjusted one to the other in some more inclusive "mode of behavior" which will coincidentally constitute some adaptive reaction *and* a cleared idea of the whole affair. This process of intercurrent adjustment means, however, mutual inhibition or conflict

between the two aroused attitudes and, therefore, an emotional seizure—apprehension or fear and, at the same time, the notion of something-to-be-afraid-of. The set to run breaks over into overt running but not, perhaps, in full discharge, for the other attitude of expectant curiosity may still partially persist. At last, however, the developed idea of the object bursts into clear relief as a dangerous bear and the now terrified man legs it for all he is worth; the original attitude of expectant investigation goes entirely and the running is unimpeded. Emotion disappears in so far—but *just in so far*—as the running is untroubled by even the incipient presence of contrary attitudes or tendencies. If the total reaction pattern involves the element of "an overtaking bear," or a "hopelessly distant refuge," or any other intruder incongrous to the main activity, a resurgence of emotion will occur, since a still further reënvisagement of the situation is called for. Otherwise the reaction will be merely one of habitual adjustment, each part serving simply as 'means' or 'cue' to the next.

This poor bear has certainly been overworked in the literature; a due respect for the opinion of mankind be my excuse for drafting him again. Better analogous cases will occur to anyone—cases in which the full realization of the situation covers a longer time-span, comes more haltingly, slowing down the whole picture. One has only to recall, among other instances, the serial alternation of advances of fascinated interest, on the part of a young child, and the shrinking movements of retreat, when confronted by some bizarre situation. Conflict arises when incompatible attitudes of behavior are generated by a developing situation; such conflict is the condition of the emotional seizure; when the conflict is absent, or merges into unchecked adaptive activity, there is no emotion.

It is obvious that Dewey has gone far beyond his modestly announced purpose of bringing the James-Lange and the Darwinian ideas "into some organic connection with each other." He takes from James the instinctive basis of emotion (but admits a habit basis too); the conditioning of emotion by prior organic discharge; and the sensory repercussion. But he transmutes James's ideas into a quite different theory by four construction variations. First, the emotion-arousing 'object' is not initially *given* but is itself gradually constituted by the behavior; secondly, as this behavior proceeds, conflict is developed between incompatible attitudes aroused; thirdly, this conflict (or the sensory repercussion therefrom) *is* the emotion; fourthly, the con-

flict (and the emotion) can exist only when a prior part-activity (*e.g.* the attitude of expectant attention) persists alongside of a succeeding part-activity (*e.g.* the attitude of retreat)—for if one simply gives way to the other without overlap, there is only habit, not emotion. It is difficult to see, to mention only what has always seemed to me the main gap in James's theory, how James could deny emotion, as distinguished from a mere feeling of heightened vitality, to *any* sensory back-kick from reactions (*e.g.* hard physical exercise) that involve strong arousal of skeletal and visceral activity; unless, as Dewey points out, the arousing object were already apprehended as, *e.g.* 'fearful'—in which case the emotion has slipped in prior to the reaction. All four of the Dewey additions mentioned above are needed to fill this gap, and particularly the second and third. And yet some years before Dewey wrote, James specifically denies,[16] although he had seen the suggestion in Paulhan,[17] any crucial function to conflict as a condition of emotion. He says:

"M. Fr. Paulhan, in a little work full of accurate observations of detail, seems to me rather to turn the truth upside down by his formula that emotions are due to an inhibition of impulsive tendencies. *One* kind of emotion, namely, uneasiness, annoyance, distress, does occur when any definite impulsive tendency is checked, and all of M. P.'s illustrations are drawn from this sort. The other emotions are themselves primary impulsive tendencies, of a diffusive sort (involving, as M. P. rightly says, a *multiplicité des phénomènes*); and just in proportion as more and more of these multiple tendencies are checked, and replaced by some few narrow forms of discharge, does the original emotion tend to disappear."

The very admission that conflict may play some rôle in one sort of emotion, taken with the final statement in the last sentence of the quotation, shows that James had not seen implied in his own theory that which Dewey's reconstruction makes basic. Of course the original emotion tends to disappear when the checked 'multiple tendencies' get replaced by narrower forms of discharge, *i.e.* by habit. The emotion exists only during the 'checking.' Dewey observes that "this sort of inhibition the James theory not only permits, but demands—otherwise the whole relation between the exciting stimulus and the instinctive response, which is the nerve of the theory, disappears."[18]

Back of all this discussion there lurks, of course, the whole problem of the psychophysical presuppositions or working hypotheses of

[16]William James, *The Principles of Psychology*, 1890, II, 476, footnote.
[17]Fr. Paulhan, *Les phénomènes affectifs*, 2nd ed., 1901.
[18]Dewey, *op. cit.*, II, 26.

psychology. It is not my purpose to go into it, but for Dewey's defense of his own assumption that it is the organic reaction that conditions emotion (and other conscious processes) and that this is *somatic* and not merely *cerebral* reaction, I may refer the reader to his article.[19] Anyone who inclines to the hypothesis that emotions are adequately conditioned centrally, or cerebrally, will find no cogency in either James or Dewey. For others, whose bias favors the conditioning rôle of somatic behavior patterns, it is quite irrelevant whether they favor "sensory repercussion" or stark behaviorism, so far as the question of the adequacy of a conflict theory is concerned.

I had intended at this point to try to add to the plausibility of Dewey's main conception by a somewhat independent line of approach, but shall reserve the discussion for later publication. Meantime it is quite fitting to refer to a few later writers who, apparently independently, have seized hold of the conflict conception.

Titchener: "It is essential, then, for the formation of an emotion: (1) that a train of ideas shall be *interrupted* by a vivid feeling," *i.e.* by some other reaction system.[20]

Angell: "If it be true that consciousness tends to appear where the reflex and hereditary responses of the organism are inadequate to cope with the demands of the environment, we may say with equal truth that emotions appear whenever there is *conflict among the motor impulses called forth by any special situation.* Both cases demand fresh adjustments of consciousness for the securing of efficient action. *The significance of emotion as a fact of consciousness would seem, therefore, to be resident in this monitory function, represented by its compelling announcement of needed adjustments, its report of unstable equilibrium.* At all events this is evidently the part it plays, be its telology what it may, and obviously this conflict with an impediment in the course of carrying out coördinated activities is the universal occasion of its appearance."[21]

I may repeat that in the general literature of psychology Angell is almost alone in making explicit mention of Dewey's significance in connection with emotional theory; he gives with approval ("we shall certainly be wise in accepting it provisionally") a summary of Dewey's conception.[22]

Bernard: "Emotions are not the fixed or inherited conscious correlates of the instincts, as McDougall has supposed. They are, in part, conscious correlates of the modified processes which arise . . . resulting from *interruption or inhibition of dominant neural and activity processes.* . . . Thus emotion is seen to belong

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[19] *Ibid.*, 22 f.

[20] E. B. Titchener, *An Outline of Psychology*, 1900, 230. Italics mine.

[21] J. R. Angell, *Psychology*, 4th ed., 1910, 378.

[22] *Ibid.*, 382 f.

rather to the field of acquired than of inherited neuropsychic organization."[23] "Emotions . . . indicate *inhibition or delay or modification of action* instead of being the correlates of uninterrupted activity."[24] "Emotion is not the cause of activity, as is so frequently assumed, but one method of evaluating the suspended or inhibited activity for the organism or society."[25]

The next author is of especial interest because of his strong bias for introspection and against the inclusion in psychology of physiological and biological concepts, and his definite rejection of the sort of "approach to a physiological explanation" made by James-Lange.

Drever: "So far as the prosecution of the instinct-interest takes its normal course, and 'worthwhileness' passes normally into 'satisfyingness,' through the definite behaviour provided for by the neural prearrangement we call Instinct when we are speaking biologically, so far there is no emotion. But *if in any way this normal prosecution of the instinct-interest is checked 'tension' will arise*, a tension in feeling which is emotion."[26] "The illustration [here omitted] also shows us one kind of circumstance, under which the 'tension' of feeling, which is emotion, will be produced, that is, *when the urgency of the impulse is such that action cannot keep pace with it* . Another kind of circumstance, under which 'tension' will arise, is when there is no inherited provision for the precise reaction which is appropriate to a particular situation. . . . The survival value of precise reactions for particular situations is distinctly limited to a stable . . . environment. In a changing and complex environment plasticity of reaction . . . may involve a biological advantage . . . [but also] *some delay of reaction, and therefore some feeling of 'tension.'* Hence in the higher animals and man we should expect to find, as we do find, plasticity of reaction, and going along with this, and *pari passu* with it, signs of emotional development."[27] "*In what we should call the 'joy' emotions, the emotional 'tension' may arise under conditions exactly the reverse* of those under which emotional 'tension' ordinarily arises. In the ordinary case there is 'tension' because the satisfaction of the interest outstrips action, because action cannot follow with sufficient rapidity an impulse stimulated by the satisfaction already attained."[28]

In joy, Drever means, the "action" outstrips the "satisfaction of the interest" —gets ahead of, checks, the lagging process of clarifying Dewey's "object" or "idea."

These passages from Drever are significant. They unmistakably reveal the Dewey *motif* although proceeding from a mind unsympathetic to his psycho-behavioristic concepts and employing, consequently, quite different phraseology. Drever says that emotion arises when "tension" develops because of the "checking," or "de-

[23] L. L. Bernard, *Instinct*, 1924, 505. Italics mine.
[24] *Ibid.*, 506. Italics mine.
[25] *Ibid.*, 508. Italics mine.
[26] J. Drever, *Instinct in Man*, 1921, 143. Italics mine.
[27] *Ibid.*, 144. Italics mine.
[28] *Ibid.*, 192. Italics mine.

laying," or "suspending" of the "normal prosecution of the instinct-interest." Dewey could accept this if for the phrase "the normal prosecution of the instinct-interest" were substituted: the normal progress of a "total form of behavior" towards consummation. As one occasion for the advent of 'tension,' Drever acutely remarks that it will arise "when the urgency of the impulse is such that action cannot keep pace with it;" when running from the bear, the fright will continue if the bear is thought to be gaining; for in such case the 'impulse' is indeed great and the lagging legs cannot satisfy it.

Whately Smith: "The simultaneous evocation of systems of ideas containing conative tendencies towards incompatible reactions may lead to nervous impulses competing, so to speak, for mutually exclusive efferent paths and, by their reciprocal inhibition or blocking, occasioning an endosomatic disturbance, which in turn would originate afferent impulses appreciated as affective tone."[29]

Breese: "The essential condition for emotion, however, is that conflicting motor tendencies are aroused, and that these tendencies are initiated by the appearance of conscious states which suddenly block conscious activity and interrupt the continuity of our plans and purposes . . . inhibit the adaptive voluntary and involuntary coördinations which serve to carry them out."[30]

In addition to these authors who more or less explicitly center emotion in conflict of reactions, there are others whose statements more or less directly *imply* conflict or competition of reactions. Watson asserts that "the shock of an emotional stimulus throws the organism for *the moment at least* into a chaotic state."[31] It is difficult to see how a stimulus may result in a chaotic state—a 'behavior' state, of course—except *via* the arousal of conflicting behavior patterns. So, too, when James says that emotional reactions "usually terminate in the subject's own body,"[32] is it not most natural to suppose, since instincts are for James the very stuff of which emotional reactions are made and normally "enter into practical relations with the exciting object," that the somatic termination is due to the tension of incompatible tendencies (Dewey's "attitudes") aroused? If not mutually checking one another they would issue in their normal discharges towards the 'exciting object.' Bentley does not refer to emotion as rooted in conflict; but he speaks of the "emotive predicament," and certain examples rather imply a basic conflict, *e.g.* the diffident boy who "sits and gapes . . . shrinks from being conspicuous

[29]W. W. Smith, *The Measurement of Emotion*, 1922, 153.
[30]B. B. Breese, *Psychology*, 1917, 391 f.
[31]J. B. Watson, *Psychology from the Standpoint of a Behaviorist*, 2nd ed., 1924, 216.
[32]William James, *The Principles of Psychology*, I, 443.

. . . would like to explain . . . how can he? . . . miserably fidgets and suffers."[33] Calkins quotes Angell's account, with approval, of the way in which "the stimulus of the emotion—whether external object or image—checks the reaction of the preceding movement."[34] I do not mean to imply, by these last samplings, that the authors would at all accept the view that emotion is *born* in conflict; far from it, but the references do illustrate the extent to which various writers have seen that conflict is involved somewhere along the line. The problem is to *place* it. Is it constitutive, or incidental, or only a result?

Dewey, I feel, is right. But the thesis might be stated somewhat differently, as follows: Whenever a series of reactions required by an organism's total 'set' run their course to the consummatory reaction which will bring 'satisfaction' (*i.e.* abolish the 'set') unimpeded by other reactions *that cannot be readily absorbed by* (*integrated with*) *them*, there is no emotion. Emotion arises only when these other reactions (implicit or overt) are so irrelevant as to resist ready integration with those already in orderly progress towards fruition. Such resistance means actual tension, checking, interference, inhibition, or conflict—conflict between the intercurrent reactions appriate to the original 'set' and reactions of other origin threatening the abortion or frustration of the relevant series. Such conflict constitutes the emotion. Whether one says it *is* the emotion, or gives rise to a sensory repercussion which *is* the emotion, or has some other organic effect which directly *conditions* the emotion, is not germane to the main issue. *Without* such conflict there is no emotion; *with* it, there is.

Adequate support for the soundness of this conception could be given only by detailed analysis of concrete emotional situations and by a seriatim consideration of objections. What rôle does the summation of stimuli play? How about joy and other pleasantly toned emotions? What of the quieter or subtler emotions? What determines the varieties of emotion? I feel that the conflict formula can readily digest these, and other, features of the problem.

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[33]Madison Bentley, *The Field of Psychology*, 1925, 305.
[34]M. W. Calkins, *A First Book in Psychology*, 1910, 206.

# THE TEMPERATURE SPOTS AND END-ORGANS

By Karl M. Dallenbach, Cornell University

This study[1] was undertaken to determine whether, under any condition or with any degree of refinement of technique, maps of cold and warm spots localized upon the surface of the skin were capable of repeated verification. As the study progressed and exact localization of the spots seemed possible, the opportunity was taken to try to identify the functional end-organs of cold and warmth, and thus to test histologically the validity of Von Frey's correlation.[2]

## I. Localization and Mapping

The central problem of this study—localization and mapping of the temperature spots—grew out of our failure in the Cornell undergraduate laboratory to obtain a high degree of correlation between the results of successive mappings.

The localization and mapping of the cold and warm spots is usually the first experiment performed in the qualitative laboratory course at Cornell. The students are instructed to work over the area of the skin selected for investigation—customarily, for the sake of convenience, an area in the region of the second and third metacarpals—first with cooled, and then with warmed metal cylinders, and to map the spots at which temperature experiences are aroused.[3] At a later sitting the procedure is repeated to verify the results of the first experiments. The correspondence between the maps obtained at the different sittings is, as the results during a period of more than ten years show, not much above 30%. In some instances, particularly in the case of the warm maps, correspondence is very low.

I give in Table I, by way of illustration, the results obtained in one of my laboratory classes. The correspondence for cold maps ranged between 17.8–39.3%; and for warm maps, between 15.4–33.3%. It was determined by computing the percentage of spots on the second map that corresponded to spots on the first; spots were counted as 'corresponding' if they were localized upon the two maps within 1 mm. of each other. If there were 28 spots, for example, on the

[1]The experiments here reported were begun in the fall of 1916. A preliminary report of the results was rendered at the 1917 meeting of The Experimental Psychologists at Harvard, and a full report at the 1926 meeting of the American Psychological Association at Philadelphia.

[2]M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. kgl. sächs. Ges. d. Wiss., math.-phys. Kl.*, 47, 1895, 181-184.

[3]For the procedure see E. B. Titchener, *Experimental Psychology, Qualitative, Student's Manual*, 1901, 54 ff.

second map and 8 of these corresponded to spots on the first map the correspondence would be 28.6%.[4] Because I shall have something to say later about methods of computing correspondence, the method here described will henceforth be referred to as the 'student method.'

The lack of a high degree of correspondence, however, was regarded by the instructing staff as of small moment, for the principal objects of these experiments were "to convince the student that the spots are not artifacts, products of the imagination,"[5] and to afford him opportunity "to become introspectively familiar with temperature sensations in their purest form."[6] Moreover, the discrep-

TABLE I

RESULTS OF MAPPING BY STUDENTS

Area 4 sq. cm.

| *O* | Cold | | | Warm | | |
|---|---|---|---|---|---|---|
| | 1st map | 2nd map | % cor. | 1st map | 2nd map | % cor. |
| E R | 24 | 28 | 28.6 | 6 | 9 | 22.2 |
| N G | 28 | 18 | 33.3 | 12 | 10 | 20.0 |
| M P | 24 | 28 | 32.1 | 9 | 13 | 30.7 |
| M C | 28 | 31 | 32.2 | 16 | 19 | 26.3 |
| L R | 25 | 26 | 34.5 | 17 | 19 | 26.3 |
| B S | 20 | 30 | 23.3 | 5 | 6 | 16.6 |
| J A | 38 | 28 | 39.3 | 9 | 13 | 15.4 |
| E S | 40 | 28 | 17.8 | 9 | 10 | 33.3 |

ancy between the maps, *i.e.* the low correspondence, was explained by the crudity of the method of investigation and the inexperience of the observers. If a more precise technique and trained observers were used, the students were told, a higher degree of correlation would be obtained. How much higher, none of the instructing staff ventured to say; but in the minds of all certainly, correlation was expected to be not far short of 100%.

Two members of the staff,[7] not satisfied with the indefinite answer "a higher degree," determined to find out for themselves 'what degree' training and a precise technique would give. They were highly trained in introspection, and they used the best technique and apparatus available. The surprise of the staff was very great, therefore, when they failed to obtain perfect correspondence—or anything approximating perfect correspondence.[8] Their correlations ranged be-

[4]This method of computing the correspondence, it should be noted, leaves out of account the spots on the first map that have no correlates on the second. Nor does it take into consideration the areas in the two maps that give no response—yet here is correspondence of the strictest kind. This method of computing correspondence has nothing but ease to recommend it!

[5]Titchener, *op. cit., Instructor's Manual*, 1901, 83.

[6]*Idem, Student's Manual*, 54.

[7]W. S. Foster and E. G. Boring.

[8]Their results confirmed Tigerstedt's conclusions that "eine völlig exakte Bestimmung der Lage der Temperaturpunkte ist . . . sehr schwierig, wenn überhaupt möglich" (Robert Tigerstedt, *Lehrbuch der Physiologie des Menschen*, II, 1902, 86).

tween 50 and 60%—very much higher than those obtained by the students, but far short of the ideal. Out of the discussions that revolved around these experiments and results, grew the study here reported.

The theoretical importance of the localization of the temperature spots, is very great. If the spots cannot be definitely localized, if successive surveys always yield different maps, if there is no consistency in the localization of the spots, then—assuming the technique to be faultless—our theories of cutaneous sensation would have to be revised, and our theories of nerve excitation and neural conduction would have to be considerably modified.

The technique, however, was very soon discovered not to be without faults. Indeed, this investigation might well be called a 'study in errors,' for it quickly resolved itself into a problem of obtaining an adequate technique for the work. One kind of apparatus after another, and one method after another, were tried and discarded. It was only after many failures that an apparatus and a procedure were devised that gave unequivocal results. An account of how the method developed will not only demonstrate the necessity for the safeguards finally adopted, but it will also point out the technical pitfalls that must be avoided in this field of research.

## A. Preliminary Experiments

All the experiments with apparatus and procedures that gave equivocal results were considered as preliminary and they are therefore reported under this heading.

*Apparatus.* A thermo-aesthesiometer of the Kronecker-Miescher type[9] was used in the preliminary experiments. The tip of the cone, which served as the stimulus-point, was rounded and was 0.9 mm. in diam. Rubber tubing was attached to the inlet and the outlet of the instrument, and water at a constant temperature was forced through it by gravity. The water supply was obtained from reservors elevated about 1 m. above the experimental table. The cold reservoir was filled with cracked ice. The warm reservoir was heated by a Bunsen burner controlled by means of a Roux regulator.[10]

The temperature of the water-bath was taken before it entered and after it had passed through the stimulator. The average of the two readings was assumed to be the temperature of the stimulus. After the water-bath had been brought to the desired temperature it was remarkably constant, varying less than a degree within a given experimental hour. The temperature for the different experimental hours varied within greater limits: for cold from 5–9°, and for warmth from 41–44°C.

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[9]C. S. Sherrington, Cutaneous sensation, in Shäfer's *Text-Book of Physiology*, 1900, 948.

[10]Titchener, *op. cit.*, *Instructor's Manual*, 83.

*Observers.* The *O*s in the preliminary experiments were Dr. E. G. Boring (B), instructor in psychology; Miss Josephine M. Gleason (G) and Mr. Gilbert J. Rich (R), graduate students in psychology; and the author (D).[11] The *O*s served three times a week and always at the same hour of the day. Since the *O*s were all following a fixed schedule in their daily programs, they brought to the experiments on the different days a cutaneous surface that had been subjected to approximately the same amount of adaptation and fatigue.

*Procedure.* A grid 2 cm. square, with cross-section lines 2 mm. apart, was stamped in indelible ink upon the area to be investigated. The location of the hairs was then noted with the aid of a large reading-glass, after which the hairs were shaved off so as to give as uniform a skin surface as possible.

After the cylinder was brought to the desired temperature and the *O* was seated in a comfortable position opposite *E*, the stimulus was applied to the stamped area by the 'continuous' method, *i.e.* the point of the stimulator was applied lightly to the skin and drawn slowly across the surface. Care was taken to see that the cylinder was at all times perpendicular to the skin, and that the pressure and the rate of movement were as uniform as possible. The stimulus was always moved in the same direction during the experiments of a given hour. *E* followed the first line across the stamped area, then the space between the first and second lines, then the second line, then the space between the second and third lines, then the third line, and so on, until the entire area had been worked over. The spots at which *O* reported temperature experiences were noted and entered on a map—a duplicate of the grid. The cold stimulator usually was used first;[12] then after a rest of 10–15 min. the warm cylinder was used and the warm spots mapped. At a subsequent period the procedure was repeated and a second set of maps obtained. The percent of correspondence of the second maps to the first was then computed.

*Areas.* In the preliminary investigations three different areas of the skin were used: (1) an area on the back of the left hand between the second and third metacarpals; (2) an area on the dorsal side of the left forearm, 10 cm. from the ulnar prominens; and (3) an area in the middle and exterior part of the left upper arm.

*Results.* (*1*) *Hand.* The area on the hand was the first mapped. It was chosen because of convenience and ease of stimulation. The results, however, did not justify its selection. The correspondence of the second set of maps to the first was far from perfect, ranging for the different *O*s from 42–73%.

The low correspondence was due, not so much to defects in technique—although as it later appeared they were numerous—as to the fact that the skin on the back of the hand is an extremely poor tissue for exact work. The subcutaneous tissues, composed of the metacarpal bones, phalangeal tendons, loose connective tissue, and superficial blood vessels, offer a very heterogeneous background upon which to work. The temperature cylinder, as it is drawn across this area of the skin is now near a superficial vein, now over a metacarpal bone, now crossing a tendon, now sinking into the soft interstices between the bones. These differences

[11] My thanks are due Professor Paul Thomas Young, then fellow in psychology, for acting as *E* when I served as *O*.

[12] The cold stimulus was used first so that if there were any effects of 'tuning' they would be in the direction of increased sensitivity.

in themselves would not be serious if the skin maintained a constant relation to the subcutaneous tissues. Unfortunately, however, such is not the case. The slightest alteration in the position of the fingers or the tension at which the hand is clasped changes the relationship. Even if *O* could maintain a constant position and tension of the muscles of his hand during the course of a single mapping, he could only by the merest chance duplicate the conditions at a second sitting. The excitation and localization of a temperature spot is a function of the adequacy of the stimulus; and the adequacy of the stimulus varies with a change in the nature of the subcutaneous tissues. If spot *a* is over a vein during the first mapping, and over a bone during the second mapping—to take the most extreme variation—the effect of the stimulus as it passes over *a* will be very different in the two cases. Moreover, the pressure of the stimulus-point, though every effort be made to keep it constant, will vary as it passes over the different kinds of subcutaneous tissues. Discrepant results in successive maps of such a varying tissue are not surprising; indeed the correspondences that we did obtain are, under the circumstances, more surprising than the failures.

The net result of the experiments on the back of the hand was a greater appreciation of the need for constant conditions. We had the choice, therefore, of trying to control the variable errors met in these experiments or of changing the locus of the investigation to a part of the skin in which these errors did not exist. We chose the latter course.

(*2*) *Forearm.* Because the tissue of the forearm was thought to be free from the variable errors met in the experiments on the hand, and because the forearm was easy to stimulate and convenient for the *O*s, a section on the dorsal side of the arm, 10 cm. from the ulnar prominens, was the next area mapped.

After a few sittings, at which correspondences of about 60% were obtained, work on this area was abandoned, for we discovered that the forearm did not meet the requirements of the work. In the first place, the relation of the skin and the subcutaneous tissues is not constant. It varies with the shape and size of the muscles, which vary inturn with every movement and change in position of the hand and fingers. We could not hope that the *O*s would duplicate, at different experimental sittings, the original position of their hands and fingers. Indeed, we could not even expect the *O*s to maintain a constant position throughout the course of a single experimental hour. In the second place, and more particularly (for we might have devised some means of controlling the positions of the hand and fingers), the hair on this region of the *O*s' arms was so thick and so evenly distributed that we could not hope, even if we did obtain accurate and verifiable maps, to test Goldscheider's contention that the temperature spots radiate from centers coincident with the papillae of the hair.[13]

Ease and convenience are poor guides in the selection of an experimental tissue.

(*3*) *Upper arm.* After considering, in the light of our experience on the hand and forearm, the various other surfaces of the body that might be used, we turned to an area in the middle and exterior part of the *O*'s left upper arm.

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[13]Alfred Goldscheider, Ueber Wärme-, Kälte-, und Druckpunkte, *Arch. f.* [*Anat. u.*] *Physiol.*, 1885, 340-345.

The tissues on the upper arm are homogeneous, there are no tendons there, or bones, or superficial blood vessels to interfere with stimulation. This area, furthermore, has relatively few hairs. The relation of the skin and the subcutaneous tissues is also comparatively constant. Even gross movements of the hand and fingers leave the tissue relations of the upper arm unaffected.

The angle at which the arm was bent at the elbow, was the only variable we foresaw that would affect the relationship of the different tissues. We thought to control this by making plaster-cast arm-rests for every *O*. The casts were made of *O*'s entire arm, from the shoulder to the fingers, so the position of the arm was approximately constant throughout the course of a single hour and at the different experimental sittings.

The *O* was seated in a comfortable chair besides a table with his arm in his plaster cast. The chair and table were fastened to the floor, and *O*'s position in the chair and the position of his arm-rest upon the table were marked, so that these factors could be duplicated at successive sittings.

In spite of all these precautions, we obtained correspondences between successive maps of only about 50%—poorer than the correspondences obtained upon the back of the hand and the forearm.

The failure to obtain a higher degree of correspondence was due in part to our inability to maintain the experimental grids upon the skin. The grids, though printed in indelible ink, were almost obliterated during the intervals at which we worked. We tried many inks and stains, but the abrasion of the clothes, or the absorption of the skin, or both, effaced the grids sufficiently to introduce a large variable error in the localization of the different sensory spots.

We tried to revive the faded grids by restamping, but, for reasons which we shall presently report, we could never get the second stamping to correspond with the first. We thought that since the stamp slightly obscured our vision the failure was due to our inability to place it exactly upon the position of the first impression. We, therefore, resorted to a stamping apparatus devised by Boring for his work in the study of cutaneous sensation in nerve regeneration.[14] The rubber stamp of this apparatus (which is shown in Fig. 1) is on a stage that moves vertically upon the four legs of a small table. The position at which the stamp makes a print is constant in relation to the legs. If the legs are brought to a constant position a duplicate grid—on paper at least—can always be made. Since the legs are long and pointed, there is nothing to obscure *E*'s vision, and *E* can always bring the legs down to a designated position.

The points on which the table legs rested when the grid was printed upon the arm with this apparatus, were tattooed into the skin. We thought, therefore, that the problem of maintaining the experimental maps was solved. When we turned, however, to use this special apparatus we found that the tattooed spots did not coincide with the points of the table legs. All possible variations in the positions of the spots were noted. Instead of occurring at the corners of a square, as we had expected, the spots marked the corners of an irregular quadrilateral which differed in shape at every experimental sitting. The only time that the spots coincided with the legs of the table was when they were tattooed.

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[14]E. G. Boring, Cutaneous sensation after nerve-division, *Quar. J. Exper. Physiol.*, 10, 1916, 1-95.

The positional variation of the spots was caused by the volumetric changes of the arm. The volumetric change, having thus come to our attention, was observed also in the fit of the *Os*' arms in their plaster casts. At times the *Os*' arms fitted their casts snugly, at other times loosely.

The changes in volume account for our failures to revive the grids by restamping, and thus indirectly for the poor degree of correspondence between the successive mappings. Since a much greater mass of tissue was involved in the upper

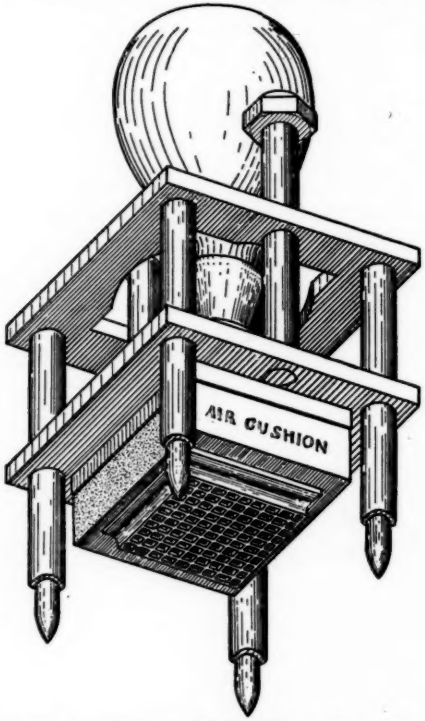


FIG 1. BORING'S PRECISION CROSS-SECTION STAMP (Actual Size)

arm, the volumetric changes also account for the fact that poorer correspondences were obtained between the successive maps of the upper arm than between the maps of the forearm or hand.

Since we knew of no method of controlling volume, we abandoned our efforts to revive the faded grids and turned our attention to discovering some method of preserving them, at least for the duration of the experiment.

We tried to protect the grids by using various kinds of coverings. Bandages of cloth and gauze would not suffice, for, with the constant movement and interplay

of the muscles of the arm, they had the same effect as the clothes. Vaccination caps were used as protective coverings, and though they preserved the grids they otherwise proved unsatisfactory. The skin under and around the caps became inflamed, and we found after a few days that we were working with a slightly abnormal tissue. We could not tell what was responsible for the irritation; it might have been caused by any one or by all three of the following factors: (1) by the adhesive that we used to fasten the caps to the skin—we used, at different times, surgical tape, several kinds of glue, and celloidin; (2) by the method of removing the caps when starting work at an experimental sitting—at first we jerked them off, but as this proved very irritating we later softened the adhesives before lifting the shields; (3) by the confinement of the skin—since the caps we used were without vents and were glued tightly to the skin, there was no ventilation and the normal functioning of the skin of the experimental area might have been interfered with.

The easiest of these three possible causes of irritation to correct, and the first one we turned to, was the lack of ventilation. When we were punching ventilation holes in the caps, it occurred to us to cut a large hole in the center of every cap, so that the skin could be stimulated by the temperature points without the necessity of removing and replacing the shields at every experimental hour. This idea led to the abandonment of the caps and to the substitution of shields made of thick felt rings.

The felt rings were fastened with celloidin to the skin close around the grids, which were reduced in size from 4 to 1 sq. cm. The rings preserved the grids by keeping the clothes from rubbing the stamped areas, they allowed normal ventilation, and they permitted free stimulation of the experimental area by the temperature points. Thus at once two of the possible causes of irritation were removed.

In the case of three of our *Os* (D, G, and R) irritation was in fact eliminated when we used the felt rings as shields; but in the case of the fourth *O* (B) it was aggravated. The inflammation increased and the tissues within and around the felt ring became swollen. We could not ascertain the cause of this; we used different kinds of adhesive—as mentioned above—but in every case the effect was the same. It was probably due to a peculiar tenderness and susceptibility of B's skin to the constriction that even the felt rings produced under the varying tensions and pressures caused by the volumetric changes of the arm. Experiments with B were therefore discontinued after the preliminary work.

## B. Sources of Error Noted in the Preliminary Experiments

We noted in the preliminary experiments the following sources of error: (*1*) changes in the relation of the cutaneous and sub-cutaneous tissues; (*2*) volumetric changes of the tissues; (*3*) the instability of the grids stamped upon the skin; (*4*) the elasticity and stretch of the skin under the point of the temperature cylinder; (*5*) the difficulties of localization; (*6*) the pressure of the stimulus; (*7*) the duration of the stimulus; (*8*) radiation; (*9*) summation; (*10*) the positive after-image; (*11*) fatigue; and (*12*) 'tuning.'

(*1*) *Changes in the tissue relations.* The relation of the skin to the subcutaneous tissues changes with every movement. The more heterogeneous the tissues, as those of the back of the hand, the greater is the effect of movement; but even relatively homogeneous tissues such as those of the upper arm are affected by positional changes, for the adequacy of the temperature stimulus depends in part upon the nature of the subcutaneous background. If a given 'spot' on the skin (to take an extreme case) is over a bone at one experiment, over a blood vessel at the next, over a tendon at the third, and over soft connective tissue at the fourth, the effect of the stimulus was found to differ greatly in the four cases. Neither the pressure nor the temperature differential would be constant upon these four backgrounds. As described above, we controlled this variable by using relatively stable tissues and plaster-cast arm-rests.

(*2*) *Volumetric changes.* Due to causes that we were unable to ascertain and to control, the volume of the tissues, particularly those of the upper arm, changed slightly from day to day. Whether these changes affected the 'tuning' and functioning of the temperature end-organs we are unable to say, but they did complicate the problem of localizing the temperature spots. The grids, which were perfect squares when stamped upon the skin, were distorted in every direction. As long as we worked with the original grids—though distorted—and localized the temperature spots upon them, the volumetric changes did not greatly affect the results. Since a grid once stamped upon the arm could not—because of the volumetric changes—be replaced if smudged or effaced, our problem became one of maintaining the original imprints for the duration of the experiment.

(*3*) *Instability of the grids on the skin.* The abrasion of the clothes and the absorption of the skin rendered it difficult to retain the original grids upon the skin. Yet, because of the volumetric changes, the original grids must be retained. After many attempts we solved this problem, as described above, by using shields made of rings of felt.

(*4*) *Elasticity of the skin.* When the temperature-point is drawn across the surface of the skin by the 'continuous' method, the skin, which is elastic and loosely connected to the subcutaneous tissues, tends to be displaced in the direction of the movement. This not only adds to the difficulty of localizing the 'spot' discussed in (*5*) below, but it also augments the errors caused by the changes in the tissue relations discussed in (*1*) above. The errors due to the elasticity and stretching of the skin could be avoided if a discrete or 'punctiform' method of stimulation were used in place of the 'continuous' method.

(*5*) *Difficulties of localization.* The difficulty of localizing the exact 'spot' at which the sensory experience was aroused is very great when the 'continuous' method of stimulation is used. This is due in part to the displacement of the skin as mentioned in (*4*) above, but it is principally a matter of reaction-time. The stimulus-point, for example, passes over a sensory spot, the end-organ responds, an afferent impulse is set up, *O* experiences the sensation, gives his report, *E* apprehends it, and then notes the position of the stimulus-point and marks it as the 'spot' at which the sensation was aroused. During the time interval of these reactions, approximately $\frac{1}{5}$ sec. if we regarded them as two simple sensory reactions (which of course they were not, as neither *O* nor *E* were under reaction-instruc-

tions),[15] the stimulus-point was continuously moving. The 'spot' that *E* marked would be displaced, therefore, from the 'true' spot, and the amount of displacement would vary according to the rate at which the stimulus was moving, the reaction-time of *O*, and the reaction-time of *E*. We had no objective control of these moments, but it is certain that they were not constant, and that a variable as well as a constant error was involved in the localization and plotting of the 'spots.'

The temperature stimulator, moreover, was not adapted for fine discriminations in localization. Because the conical point of the water-bath chamber was blunt, the apical tip which served as the stimulus-point was not sharply delimited and *E*'s vision of it was partially obscured. *E* could not therefore determine exactly the position of the stimulus at the time of *O*'s report.

The probable error of *E*'s localizations is a fourth source of error that must be taken into account when comparing the successive maps. Assuming that every other source of error was controlled, the probable error alone would suffice to introduce discrepancies. They would be small, to be sure, but they would be finite, consequently we should never get absolutely identical maps.

The first two difficulties of localization are inherent in the 'continuous' method of stimulation—both would be eliminated if the 'punctiform' method were used. The size of *E*'s probable error in localizing the 'spots' would, moreover, be smaller in the 'punctiform' method, and it would be decreased still further if the point of the temperature stimulator were so constructed that *E* could more readily determine its exact position on the skin at the time of *O*'s report.

Another source of error in the 'continuous' method is pointed out by Titchener. He says, "errors are apt to be made in localization, by the fact that *approach* to a cold or warm spot will set up a weakly cold or warm sensation. Even after repeated cautions, a student is likely to enter this approximation-sensation in his maps rather than the more intensive sensation which he can obtain by moving the cylinder a little farther, until it lies exactly over the temperature spot."[16] 'Punctiform' stimulation would reduce the variability of such localizations, and training the *O*s would eliminate it.

(*6*) *Pressure.* It was utterly impossible, by the 'hand and eye' method that we used, to carry the stimulator across the experimental area without varying the pressure of the stimulus-point upon the skin. Since the adequacy of the stimulus is in part determined by the pressure with which it is applied, it became incumbent upon us to devise some objective means of controlling this variable.

(*7*) *Duration.* The duration of the stimulus, in the 'continuous' method, is a function of the rate and uniformity of movement. Though we tried to move the stimulus-point across the skin at a steady and uniform rate, it was obvious that we were unable to do so. Not only did the absolute time of the movement across the skin vary, but, because of the elasticity, looseness, and stretching of the skin mentioned in (*4*) above, the rate of movement within any one transit was not always uniform. Since the adequacy of a temperature stimulus is in part determined by the duration of the stimulus, the necessity of controlling this variable is quite evident.

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[15] A. Goldscheider, Ueber die Reactionszeit der Temperaturempfindungen, *Arch. f.* [*Anat. u.*] *Physiol.*, 1887, 468-472.

[16] Titchener, *op. cit.*, *Instructor's Manual*, 82 f.

(*8*) *Radiation.* A temperature spot may be stimulated by radiation. If this occurs and the 'spot' is localized by the position of the stimulus-point, a large variable error is introduced into the work. The possibility of stimulation by radiation was present in all our preliminary experiments. Indeed we think this source of error is inherent in the 'continuous' method of stimulation. But over and above this methodological error our apparatus was not designed with this source of error in mind. The apical angle of the conical temperature chamber, the tip of which was used as the stimulus-point, was large. This not only hindered accurate localization of the point as discussed in (*5*) above, but it increased the liability of stimulation by radiation. For exact work the possibility of stimulation by radiation should be eliminated.

(*9*) *Summation.* Unlike the action of separate and discrete stimuli, every one of which was effective in its own right, stimulation by the 'continuous' method has a varying effect—depending on what has gone before. As the temperature-point is drawn across the skin, the temperature differential between the stimulus and the skin is affected by radiation, as discussed in (*8*) above; and the temperature experiences aroused are partially obscured by the positive after-images discussed in (*10*) below. Both these factors are summative in their effects. The more prolonged the stimulation, the greater the effects of radiation and the greater the change in the temperature differential; and the greater the number of temperature experiences aroused, the greater the number of positive after-images and the greater their obscuring effect.

Punctiform stimulation, and a proper regard for the duration and rate of stimulation would avoid the errors due to summation.

(*10*) *Positive after-images.* Positive after-images of both cold and warmth during the course of the experiments were reported by all the *O*s. To what extent, if any, they influenced the results we cannot say; none of our *O*s, as far as we know, confused an after-image with a sensation in their reports, though of course there was always that possibility with the 'continuous' method of stimulation. More serious than this possibility, however, is the distraction of *O*'s attention with a resulting increase of the reaction-time of his subsequent reports—a source of error discussed in (*5*) above.

If the distracting effect of the after-images is to be avoided the 'continuous' method of stimulation must be given up. The stimuli should be applied discretely; and they should not follow one another until the effects of the previous stimulations have run their course.

(*11*) *Fatigue.* The size of the area that we used in the preliminary work was 4 sq. cm. (2 x 2 cm.). The felt shields described above (p. 409) would not protect an area of this size—the central part of the stamped area was obliterated before the conclusion of the series of experiments. But over and above this practical consideration was the question of fatigue. Though seated in a comfortable chair and in a comfortable position, the *O*s became fatigued, restless, distracted, and ill at ease before the entire area had been worked over.

The errors of 'fatigue' could be reduced by increasing the rate of stimulation, or by decreasing the extent of the experimental area. An increase in the rate of stimulation would augment the errors discussed in (*4*), (*5*), (*7*), (*9*), and (*10*) above, whereas a decrease in the size of the experimental area would leave these

sources unaffected, and would bring no others in its train. The verification of the maps of small areas would be as convincing as that of large, accordingly the experimental area was reduced to 1 sq. cm.

(*12*) *'Tuning.'* The temperature spots are not all equally sensitive. They are 'tuned' or adjusted so that they respond differently to a given stimulus. Unless, therefore, the 'tuning' is constant, the adequacy of the stimulus may vary even though the stimulus itself be constant. Not much is known of the phenomenon of 'tuning' but it is probably dependent upon the temperature of the skin, *i.e.* the physiological zero, and the general health of the *O*. We attempted to control both of these factors: the general health of the *O*s by having them, during the Main Experiments, follow a fixed routine; and the temperature of the skin by controlling the temperature of the experimental room. The observation hours were held regularly three times a week and at the same hour of the day, consequently the *O*s came to the different sittings with approximately the same degree of fatigue and temperature adaptation.

The reason that high degrees of correspondence between the results of successive mappings of the cold and warm spots have not heretofore been attained is evident. The apparatus, technique, and method of procedure were inadequate to the problem. The correspondences that have been obtained are, in view of the many sources of error that we have pointed out, more surprising than the failures.

## B. Main Experiments

We tried in the final series of experiments to make the mapping of the temperature spots a mere matter of stimulus and response. To this end we attempted to eliminate all the errors noted in the preliminary work: we reduced the area explored; we devised a new thermal stimulator; returned to the punctiform method of stimulation used by Blix in his pioneering work;[17] and modified the procedure.

*Apparatus.* In the final series of experiments a new thermo-aesthesiometer was used that was designed with the particular needs of our problem in mind. This apparatus is shown in outline in Fig. 2. As it has been fully described elsewhere,[18] a statement of the salient features will here suffice.

The stimulus-point *A*, a copper cylinder 1 mm. in diam., projects 3 mm. from the point of the cone-shaped temperature chamber *B*. The extension of the point makes for more accurate localization—the point is sharply demarcated and is not obscured by the housing of the temperature chamber; and it also reduces, if it does not actually eliminate, stimulation by radiation from the temperature chamber. The temperature of the stimulus-point is controlled by a current of water carried to chamber *B* by tube *C*, and away by tubes $D_1$ and $D_2$. These conducting tubes form the framework for the handle of the apparatus. The handle

[17]Magnus Blix, Experimentelle Beiträge zur Lösung der Frage über die speci fische Energie der Hautnerven, *Zsch. f. Biol.*, 20, 1884, 141-156.

[18]K. M. Dallenbach, Some new apparatus, *Amer. J. Psychol.*, 34, 1923, 92-94.

$F$, which is carried by the central tube $C$, is tapered at the lower end so its position as it is moved up and down tube $C$ may readily be observed on scale $H$. Spring $G$, which stands between the handle and the connecting bar $E$, carries the weight of the apparatus when it is held in a position ready for stimulation. The spring and the sliding handle make it possible to control the pressure of the stimulus. When the stimulator is applied the weight of the apparatus is gradually transferred from the spring to the skin. The proportion of the weight transferred is a function of the release of the spring, *i.e.* the distance the handle is lowered on scale $H$. If the handle is always lowered to a given point on scale $H$ the pressure of the stimulus on the skin will be constant.

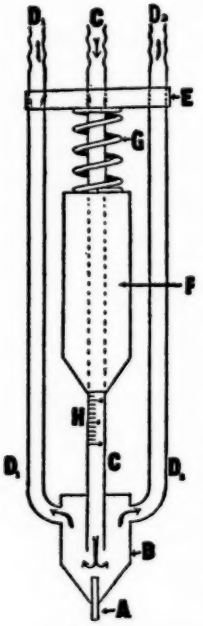


FIG. 2.
THERMO-AESTHESIOMETER
(Actual Size)

The water supply for the apparatus was obtained directly from the University mains and was forced through the stimulator and the connecting rubber tubing by the pressure of the mains. In the experiments with warmth, the water was first led through a copper coil in a constant-temperature bath, but in the experiments with cold the water was led directly to the stimulator. As in the preliminary experiments the temperature of the water was taken both before and after it had passed through the stimulator, and the average of the two readings was assumed to be the temperature of the stimulus-point. This may not have been correct, but it sufficed for our needs. We were interested not in the precise temperatures, but in their constancy and adequacy. By properly regulating the temperature of the room and the flow of the water through the apparatus, the temperature of the stimulus-point remained practically constant for any given hour—the variation for a given hour was never more than $\frac{1}{5}$°C. The temperature of the stimulus-point at the different experimental hours varied, as in the preliminary experiments, between greater limits; the cold was at 8–9°C, and the warm was at 42–44°C. The temperature of the room on the different days varied between 19.8–22.6°C.

*Observers.* With the exception of B, whose skin as we have remarked proved peculiarly susceptible to the irritation of the felt shields, the *O*s who served in the preliminary experiments continued in the final series. They were all now highly practiced in the observation of the cold and warm experiences. They observed at five periods on the main experiments—on Monday, Wednesday, and Friday of one week and Monday and Wednesday of the next—and, as in the preliminary experiments, on the same hour of the day at every sitting. Since the *O*s were all following a routine schedule, the regularity of their observation was a guarantee that they brought to the experiments a cutaneous surface that had been uni-

formly 'tuned,' that had in other words been subjected to approximately the same amount of adaptation and fatigue.

*Procedure.* After *O* had been seated in a comfortable chair with his left arm, hand, and fingers resting on a table—in a plaster-cast that had been made specially for him—a grid, 1 cm. sq., with cross-section lines 2 mm. apart, was stamped upon his upper arm. We chose an area that had relatively few and unevenly distributed hairs. After the location of the hairs was mapped, the area was shaved, and the felt-ring shields were fastened with celloidin to the skin around the grids. Nothing further was done on this day. The chair and table were fastened to the floor, and the position of *O* in the chair and of his plaster-cast on the table were marked. *O* could therefore return to approximately the same position at the different experimental hours.

The 'punctiform' method of stimulation was used. We began with the 2 mm. square in the upper left-hand corner of the experimental area and worked across the first row of squares, then we worked across the second row of squares, then

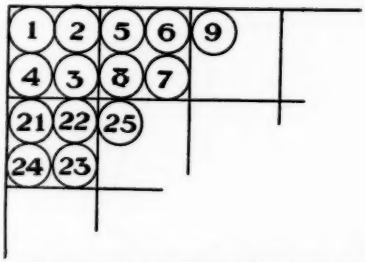


FIG. 3. PROCEDURE IN EXPLORING SUCCESSIVE SQUARES OF THE GRID

the third row, and so on. Stimulation within every square followed in the order shown in Fig. 3. The stimulus-point was placed in the upper left-hand corner of the first square, then in the upper right-hand corner, then in the lower right-hand corner, and lastly in the lower left-hand corner. After which the second square was explored in like manner, and so on until the entire area had been worked over.

The pressure and duration of the stimulus was constant throughout the course of the experiment. Uniformity of pressure was secured by means of the spring control on the stimulator described above; constancy of duration was secured by means of a silent second's pendulum that *E* kept track of by peripheral vision and subvocal counting. The stimulus was applied at the count of one, removed for the counts of two and three, applied again at one, and so on, until the first row of squares had been stimulated, at which time a short rest was taken so *O* could break his sustained attention. A 'ready,' 'now,' signal was given before the first stimulation of every row of squares, but thereafter none was given, the stimuli themselves served as their own preparatory signal.

The stimulator was applied with *E*'s right hand, with his wrist resting upon *O*'s plaster-cast arm-rest. The stimulus-point could therefore be applied steadily

and accurately. When *O* reported the arousal of a 'cold' or a 'warm,' the position of the 'spot' was marked by *E*, with his left hand, on an enlarged map, without interrupting the course or rate of the stimulus-series.

Four maps were made of both the cold and the warm spots. We had planned to make five, but the condition of the grids and the skin after the fourth sitting did not warrant a further postponement of the excision of the temperature spots —an extension of the experiment that we decided upon when it became evident that the exact localization of the spots was possible. The cold spots were mapped first at every hour, and then after a short rest during which *O* was free to move around the room as he pleased, the maps of the warm spots were made.

*Results.* The maps that were obtained at the different sittings are reproduced in Plates I, II, and III, for D, G, and R, respectively. The number of cold and warm spots obtained at every mapping is given in Table II.

TABLE II

NUMBER OF COLD AND WARM SPOTS AROUSED AT THE DIFFERENT SITTINGS
Area 1 sq. cm.

| *O* | Maps of cold spots | | | | | Maps of warm spots | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | Av. | 1 | 2 | 3 | 4 | Av. |
| D | 12 | 11 | 11 | 14 | 12.00 | 6 | 8 | 8 | 8 | 7.50 |
| G | 19 | 21 | 16 | 15 | 17.75 | 20 | 20 | 19 | 20 | 19.75 |
| R | 14 | 12 | 15 | 16 | 14.25 | 23 | 18 | 24 | 25 | 22.50 |

D reported, for the particular location that we selected, an average of 12 cold spots and 7.5 warm spots; G reported an average of 17.75 cold spots and 19.75 warm spots; and R reported an average of 14.25 cold spots and 22.50 warm spots. D shows a slight difference in favor of the cold spots, G a slight difference in favor of the warm, and R a marked difference in favor of the warm. These results stand in direct opposition to statements appearing in current textbooks that the cold spots greatly exceed the warm in number,[19] and to the results that have been obtained by the 'continuous' method in other experiments.[20] (Cf. Table I above.)

The discrepancy between our results and those previously obtained may be due to individual differences, to areal differences, or to differences in technique. Though there is large variation among the results of our *O*s, all of them report warm spots greatly in excess of the number expected. We hardly believe, therefore, that indi-

[19]Titchener (*A Text-Book of Psychology*, 1909, 150) gives the proportion of cold to warm spots as 13 to 3; Ladd and Woodworth (*Elements of Physiological Psychology*, 1911, 346) place it at 10 to 1.

[20]G. Sommer, Über die Zahl der Temperaturpunkte der äusseren Haut, *Sitzber. d. phys. med. Gesell. Würzburg*, 1901.

vidual differences will explain the discrepancy. Neither do we believe that areal differences will explain it, for the upper arm is not peculiarly sensitive to warmth, and we should expect the same relation to hold there as upon other surfaces of the skin. The method of stimulation seems therefore to be the sole basis of the results.

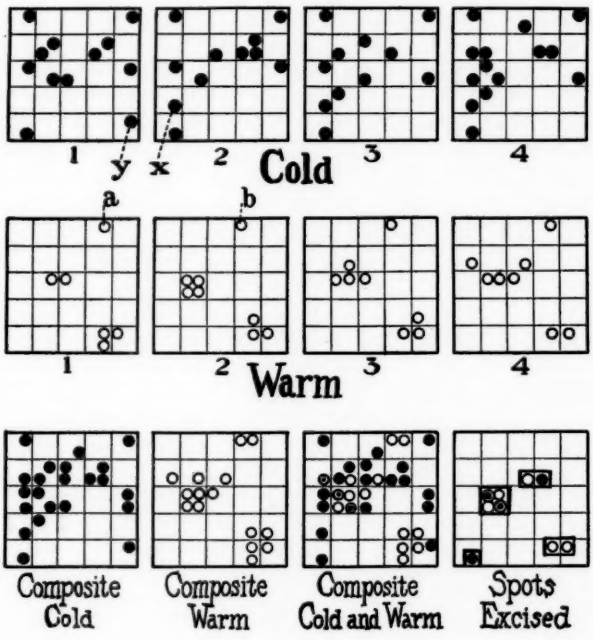


PLATE I

MAPS OF COLD AND WARM SPOTS FOR D

Area 1 sq. cm.

Whether our results are artifacts of the 'punctiform' method, or the results of other investigators are artifacts of the 'continuous' method, we are not prepared to say. It is certain, however, that the distribution of the cold and warm spots, as shown in Plates I, II, and III, is such that had the 'continuous' method been used the usual results would have been obtained. The cold spots are more or less separate and discrete, stimulation by the 'continuous' method would therefore

have separately aroused them. The warm spots on the other hand, seem to be arranged in groups. D, for example, gives three distinct clusters, and G and R each five. If these clusters are stimulated by the 'continuous' method there would be a strong tendency to consider the group as a single, large diffuse 'spot.' Which is the true picture,

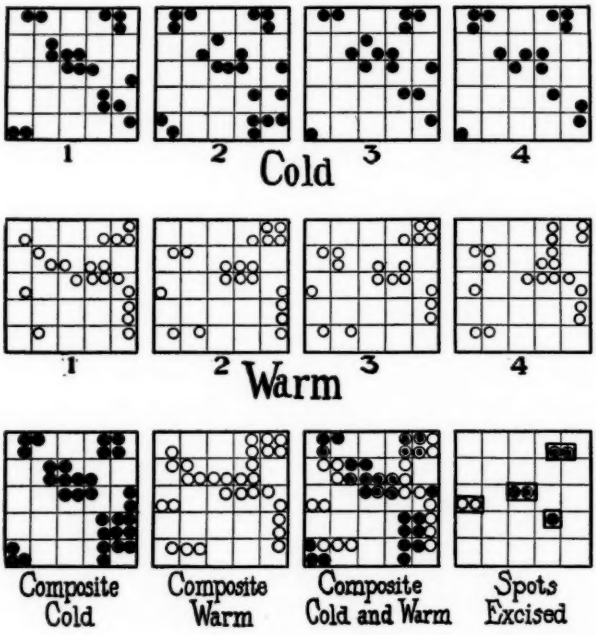


PLATE II

MAPS OF COLD AND WARM SPOTS FOR G
Area 1 sq. cm.

*i.e.* whether there are a few large warm spots or numerous small warm spots grouped into clusters, we are unable to say, for the results seem so obviously a function of the method. The answer, in our opinion, awaits the identification of the physiological correlate.

The percent of correspondence between the different maps of the cold and warm spots, is given in Tables III and IV.

## Table III

PERCENT OF CORRESPONDENCE BETWEEN THE VARIOUS MAPS OF THE COLD AND WARM SPOTS

Computed by the 'student method'

| *O* | Maps of Spots | Correspondence of | | | | | | | | | | | | Av. | | m.v. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 1–2 | 2–1 | 1–3 | 3–1 | 1–4 | 4–1 | 2–3 | 3–2 | 2–4 | 4–2 | 3–4 | 4–3 | | | |
| D | Cold | 92 | 91 | 92 | 91 | 92 | 86 | 100 | 100 | 91 | 93 | 100 | 100 | 94.0 | ± | 4.0 |
| | Warm | 100 | 100 | 100 | 100 | 100 | 88 | 100 | 100 | 100 | 88 | 100 | 100 | 98.0 | ± | 3.3 |
| G | Cold | 100 | 95 | 100 | 100 | 95 | 100 | 90 | 100 | 86 | 100 | 94 | 100 | 96.6 | ± | 3.9 |
| | Warm | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100.0 | ± | 0.0 |
| R | Cold | 100 | 92 | 100 | 93 | 100 | 94 | 100 | 100 | 100 | 100 | 100 | 100 | 98.2 | ± | 2.6 |
| | Warm | 96 | 100 | 100 | 100 | 100 | 100 | 94 | 100 | 94 | 100 | 100 | 100 | 98.7 | ± | 2.0 |

The values given in Table III were obtained by the 'student method' by which the correspondence of one map to another is determined by the percentage of spots that are identically localized or that fall within 1 mm. of one another. For example, D reported 12 cold spots at the first mapping (Plate I, Cold 1), and 11 at the second

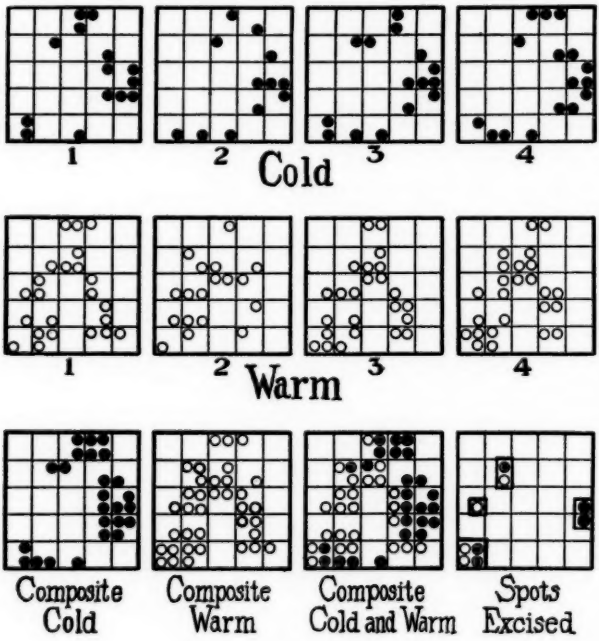


PLATE III
MAPS OF COLD AND WARM SPOTS FOR R
Area 1 sq. cm.

(Plate I, Cold 2); 11 of the 12 spots in the first map 'corresponded,' in this meaning of the term, to spots in the second map, and 10 of the 11 spots in the second map corresponded to spots in the first. The correspondence of D's first map to his second is therefore 92%, and of his second to the first 91%. The correspondence of the two maps is the average of these, 91.5%.

The correspondences for every two of the four cold maps range for D from 86 to 100%, averaging 94%, they range for G from 86 to

100%, averaging 96.6%, and they range for R from 92 to 100%, averaging 98.2%. The correspondences for every two of the four warm maps range for D from 88 to 100%, averaging 98%, they range for R from 92 to 100%, averaging 98.7%, and for G the correspondences are uniformly 100%.

It may be, in view of the many sources of error in localizing and mapping the temperature spots, that the 'student method' of computing correspondence is as precise as conditions warrant; but the method seems to us to be defective in two ways. In the first place, all the spots on the two maps between which a correspondence is being figured are not taken into account in the separate computations. Take, for example, the case of the correspondence of D's cold maps 1 and 2, mentioned above. The correspondence of map 1 to map 2 was first computed, then the correspondence of map 2 to map 1 was computed, and then the two values thus obtained were averaged, and the average was regarded as the correspondence between the two maps. But in figuring the correspondence of map 1 to map 2, the spots on map 2 were taken into account only in so far as they corresponded to spots on map 1. There is one spot in map 2 (marked $x$ in Plate I, Cold 2) that has no correspondence in map 1, and it was disregarded in the computation of the correspondence of map 1 to 2. Similarly when the correspondence of map 2 to map 1 was computed. There is one spot in map 1 (marked $y$ in Plate I, Cold 1) that has no correspondence in map 2, and it is disregarded in the computation of the correspondence of map 2 to map 1. The averaging of the correspondences thus obtained does not eliminate these separate errors; it compounds them. In the second place, the 'blanks,' the spots at which no temperature experiences were reported, are disregarded in the 'student method.' The fact that neither warm nor cold was reported at a given spot is just as significant, we feel, as a positive report. A 'blank' is a report, and the consistency with which 'blanks' appear should be taken into account in computing the correspondence of two maps.

The correspondences given in Table IV, of the cold and the warm maps, were computed by what we shall call the 'absolute method.' In this method, since there were 100 sq. mm. in every map, we simply counted the number of identical squares in the maps whose correspondence we sought. For example, there were 93 identical millimeter squares in D's first and second cold maps, 91 in his first and

third maps, 91 in his first and fourth maps, 90 in his second and third, 89 in his second and fourth, 91 in his third and fourth, and 82 of the squares were identical in all four of his maps.

Correspondence between the different pairs of cold maps ranges from 89 to 93% for D, from 84 to 93% for G, and from 84 to 93% for R. The correspondence among all four of the cold maps is 82% for D, 78% for G, and 76% for R. Correspondence between the different pairs of warm maps ranges from 92 to 96% for D, from 85 to 97% for G, and from 82 to 89% for R. The correspondence among all four of the warm maps is 88% for D, 81% for G, and 73% for R.

TABLE IV

PERCENT OF CORRESPONDENCE BETWEEN EVERY TWO, AND OF ALL THE MAPS OF THE COLD AND WARM SPOTS

Computed by the 'absolute method'

| *O* | Maps of the cold spots | | | | | | | Maps of the warm spots | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1–2 | 1–3 | 1–4 | 2–3 | 2–4 | 3–4 | All | 1–2 | 1–3 | 1–4 | 2–3 | 2–4 | 3–4 | All |
| D | 93 | 91 | 91 | 90 | 89 | 91 | 82 | 93 | 94 | 96 | 94 | 92 | 94 | 88 |
| G | 84 | 87 | 90 | 87 | 88 | 93 | 78 | 88 | 85 | 80 | 97 | 88 | 89 | 81 |
| R | 84 | 87 | 86 | 93 | 90 | 91 | 76 | 84 | 89 | 82 | 84 | 81 | 89 | 73 |

The 'absolute method' of computing correspondences between maps is, insofar as it is an 'all or none' method, inadequate to the problem. Unless the spots are identically localized upon the different maps they are not regarded as corresponding. Spots adjacently localized are not only counted as complete misses, but they reduce the correspondence more than if 'blanks' had been reported. To be specific, consider spots *a* and *b* in D's Warm Maps 1 and 2 (Plate I). These spots are adjacent. The appearance of the spot at *b* on map 2 is significant, much more so than if a 'blank' had been reported there, yet the spot at *b* reduces the correspondence, whereas a 'blank' would have increased it. This is an obvious defect that can be corrected only by adopting a system of partial credits or weights for the computation of the correspondence of adjacent spots. Since any system of weights proposed would be arbitrary, we leave the question of method with the observation that the 'absolute method' gives correspondence that are lower than the data warrant, and the 'student method' correspondences that are based upon part of the data only.

The correspondences that we have obtained between the various pair of the maps of the cold and warm spots, and among all four of the maps of the cold and warm spots, is so high for all the *O*s that we feel justified in claiming that we have demonstrated the fact that

it is possible, under standardized conditions and with an adequate technique, to obtain repeatedly verifiable maps of the cold and warm spots.

*Corollary results.* (*1*) *Warm spots definite.* The correspondences that we obtained were higher for the warm spots than for the cold. The average correspondences computed by the 'student method,' were: D, warm maps 98%, cold maps 94%; G, warm 100%, cold 96.6%; R, warm 98.6%, cold 98.2%. The correspondences among all

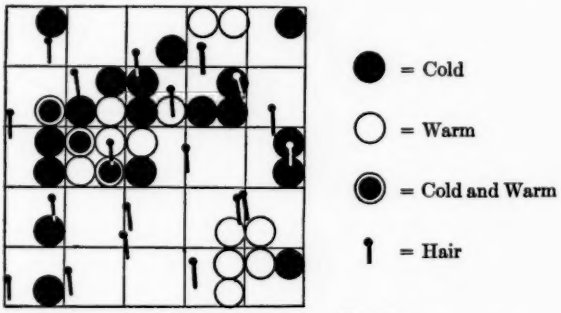


FIG. 4. D's COMPOSITE MAP OF THE HAIRS, COLD SPOTS, AND WARM SPOTS

four of the cold and all four of the warm maps, computed by the 'absolute method,' were: D, warm maps 88%, cold maps 82%; G, warm 81%, cold 78%; R, warm 73%, cold 76%. These results are contrary to what we had expected,[21] but the *O*s were consistent and their introspective reports agreed with these objective findings. During the early weeks of the experiments the *O*s gave the customary descriptions of the cold and warm qualities,[22] but as the experiments progressed and they became more proficient in the work and more familiar with the temperature qualities, the indefiniteness of the warm experiences disappeared. The *O*s found, as the objective results show, that the warm spots were more easily and more definitely

[21]Von Frey says, "Alle Beobachter stimmen darin überein, dass die genaue Lagebestimmung für die Warmpunkte viel schwerer ist, als für die Kaltpunkte" (*op. cit.*, 183). The reports of the beginning students in the Cornell qualitative laboratory, and of our *O*s during the early part of the experiment reported here, confirm this finding. The greater definiteness of the warm spots was, therefore, entirely unexpected.

[22]Titchener, *A Text-Book of Psychology*, 1909, 150; *Experimental Psychology, Qualitative, Instructor's Manual*, 1901, 82.

identified than the cold. It is only an observer who is naïve in the introspection of the qualities of the temperature spots, who describes his warm experiences as "indefinite," "vague," "illy defined," "less insistent," "more difficult to localize than the cold."

(*2*) *Map of the hairs.* A comparison of the maps of the hairs with the maps of the cold and warm spots failed to reveal any relationship between the hairs and the temperature spots. The hairs appeared to be scattered haphazardly among the cold, the warm, and the 'blank' spots on the maps of all the *O*s. A composite map of the hairs, of the cold, and the warm spots, is shown for D in Fig. 4. The composite maps of the other two *O*s are omitted for lack of space. We could find no evidence in our results, for Goldscheider's contention that the temperature spots radiate from centers coincident with the papillae of the hair.[23]

## II. The Histological Examination of the Cold and Warm Spots

When it became evident that precise and accurate localizations of the warm and cold spots were possible, we decided to test the correlation drawn by Von Frey between the Krause end-bulbs and cold, and the Ruffini cylinders and warmth.[24] Certain of the spots, therefore, that had given uniform responses in all of the mappings, were excised, fixed, sectioned, stained, and examined microscopically.

Four areas were excised from every *O*. The particular areas selected are shown in Plates I, II, and III, in the maps above the legend "Spots Excised." One of the areas contained a cold spot isolated from the warm spots, a second contained a warm spot isolated from the cold spots, and the other two areas excised contained both warm and cold spots. A composite cold map, a composite warm map, and a composite cold and warm map are shown in Plates I, II, and III, for D, G, and R, respectively. The background and the spot relationships of the excised spots can therefore clearly be seen.

The spots were excised by a surgeon.[25] The operation was performed with *O* in his usual position at the experimental table with

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[23]Goldscheider, *op. cit.*, *Arch. f.* [*Anat. u.*] *Physiol.*, 1885, 340-345.

[24]M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. königl. sächs. Gesell. d. Wiss., math.-phys. Kl.*, 47, 1895, 181-184.

[25]My thanks are due Dr. Otto E. Utzinger for his coöperation and assistance.

his left arm in the plaster-cast arm-rest. The tissue relations were therefore the same during the operation as during the mappings of the spots. Four incisions, vertical to the surface of the skin, were made on the four sides of the rectangular areas that were to be removed. The incisions extended deep (approximately 3 mm.) into the fascia; we wished to get all the tissue that might possibly contain the end-organ. The block of tissue was removed by cutting it off square at the bottom through the fascia.

The excisions were made without an anaesthetic. We could not use a general anaesthetic and have the *O*s seated during the operation in their customary positions at the experimental table—a condition which our experiences in the preliminary work seemed to indicate as very necessary. We hesitated to use a local anaesthetic because we did not know what effect it would have upon the tissue and end-organs. We did not wish at the last moment to introduce anything that might invalidate the results; so we let the *O*s suffer—which they manfully and willingly did!

The blocks of excised tissue were placed in Zenker's fixing fluid immediately upon their removal. After washing, and running them rapidly through the alcohols, they were dehydrated, imbedded in paraffin, serially sectioned (the sections were 10$\mu$ thick), and stained with methylene blue.[26]

*Results.* The microscopical examination of the serial sections revealed numerous nerves and undifferentiated nerve-endings, but not a single specialized end-organ was discovered.

Negative results, such as ours, are of course inconclusive. The logic of Von Frey's correlation is not affected by them; but as long as the experimental results are negative, and there is nothing more tangible than the logical evidence advanced by Von Frey, we feel that the psychologists and physiologists are not justified in teaching as established fact that Krause's end-bulbs function for cold, and Ruffini's cylinders for warmth.

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[26]This part of the work was done in the Histological Laboratory of Cornell University. My thanks are due Professor B. F. Kingsbury for his advice and assistance, and for the facilities of his laboratory that he placed at my disposal.

In the preliminary study of the Krause end-bulbs of a cat's conjunctiva, we used several fixing fluids—Zenker's, Hermann's, Flemming's, and 10% formalin; and several kinds of stain—methylene blue, gold chloride, hematoxylin, and picro-fuchsin. We did not observe much difference between the effects of the fixing fluids, but we were able to differentiate the Krause end-bulbs of the cat's conjunctiva more clearly with methylene blue than with any of the other stains.

Von Frey did not perform the 'crucial' experiment.[27] He had, therefore, no direct experimental evidence for his correlations. His theory is entirely a logical matter. It is based upon the fact that the end-organs which he correlates with the different sensory qualities are found in places at which those qualities are

[27]The 'crucial' experiment, *i.e.* the histological examination of the tissue under a cold or warm spot, was first performed by Donaldson in 1885. His results were negative, he failed to find any specialized end-organs. "No difference," he says, "could be made out between the spot at which cold had been felt and that at which heat was observed. There were numerous nerves beneath these spots, but these were almost as numerous in neighboring parts. The results of the histological investigation then is so far completely negative" (H. H. Donaldson, On the temperature sense, *Mind*, 10, 1885, 408 f.).

Howell says in 1905, "Many efforts have been made to determine whether there is a specific kind of end-organ for each of these senses [cold and warmth]. Numerous observers have cut out the skin from cold or hot spots and examined the removed part carefully by histological methods. The general result has been that no distinctive end-organs have been found" (W. H. Howell, *A Textbook of Psychology*, 1905, 260). These statements are repeated, without references, in every edition of Howell's book. Though I have searched long, I have been unable to find the reports of these experiments. It may be that Howell is referring to unpublished results that were privately communicated to him. But be that as it may, the second report of the 'crucial' experiment that I have been able to find was given by Häggqvist in 1913.

Häggqvist's results were also negative. He says, "In meinen auf diese Weise hergestellten Präparaten habe ich trotz genauem Durchmustern keine speziellen Nervenendkörper entdecken können, and ich ziehe daraus den Schluss, dass, welches die peripherischen Organe der beiden Temperatursinne auch sein mögen, so sind sie doch nicht Nervenendkörper, weder von Krauses noch von Ruffinis Typus" (Gösta Häggqvist, Histophysiologische Studien über die Temperatursinne der Haut des Menschen, *Anat. Anz.*, 45, 1913, 57).

Häggqvist, however, found smooth muscle bundles under every cold spot, and only under the cold spots. He says, "Ich habe in allen von mir untersuchten Kältepunkten (15 Stück) einen glatten Muskel gefunden, der in der weit überwiegenden Mehrzahl der Fälle nicht als ein Musculus Arrector pili zu deuten ist. . . . Diesen Muskel habe ich nicht gefunden, wenn ich Hautstücke, die nicht Kältepunkte enthielten, ausgeschnitten habe" (*op. cit.*, 61).

In 1925 Strughold and his colleague Karbe reported a series of studies (H. Strughold, Die Schwellen des Kältesinnes am Auge, *Zsch. f. Biol.*, 83, 1925, 201-206; Die Topographie des Kältesinnes in der Mundhöhle, *ibid.*, 515-534; Strughold and M. Karbe, Die Topographie des Kältesinnes auf Cornea und Conjunctiva, *ibid.*, 189-200; Die Dichte der Kaltpunkte im Lidspaltenbereiche des Auges, *ibid.*, 207-212; Vitale Färbung des Auges und experimentelle Untersuchung der gefärbten Nervenelemente, *ibid.*, 297-308) the results of which seem to indicate that there is a correlation between Krause's end-bulbs and the sensation of cold. The investigations were limited, however, to the conjunctiva, cornea, and the cavity of the mouth—regions in which Krause's end-bulbs are known to occur. Strughold found, for example, that Krause's end-bulbs were present in the regions of the mouth sensitive to cold, but he did not say whether they were present or absent in the regions insensitive to cold. His results leave us with an aporia.

At the 1926 meeting of The Experimental Psychologists, and again during the discussion of my paper at the Philadelphia (1926) meeting of the American Psychological Association, C. P. Pendleton reported upon a series of experiments that he had undertaken to test the Von Frey correlation between Krause's end-bulbs and cold. Pendleton worked upon the hairy regions of the body. His results were flatly negative. He not only failed to discover end-bulbs in the excised cold spots, but he also failed to discover any evidence for the occurrence of the end-bulbs in the hairy regions of the body.

especially prominent or exclusively present. He concludes, for example, that Krause's end-bulbs, which are found in the conjunctiva, the margin of the cornea, different parts of the mouth-cavity, the glans penis and clitoris, are temperature end-organs because the conjunctiva and the glans lack pressure sensation, and that they are cold end-organs because the margin of the cornea lacks warm sensations; that Ruffini's cylinders, which are found in the finger, upper arm, and eyelid, are warm end-organs because the eyelid is particularly susceptible to warmth.

Von Frey was duly cautious and conservative in stating his theory. He said, "die Endkolben sind daher wahrscheinlich die Organe der Kaltempfindung;"[28] "so scheint mir eine Beziehung der Endigungen Ruffini's zum Wärmesinn einigermassen wahrscheinlich."[29] He was, moreover, straightforward in pointing out the defects and weaknesses of his theory. He says, regarding the correlation between cold and Krause's end-bulbs, "dieser Annahme erwächst nun anscheinend eine ernstliche Schwierigkeit, weil die Fähigkeit, Kälte zu empfinden, nicht nur den aufgezählten Orten, sondern auch der äusseren Haut zukommt."[30] And again, "ob die Zahl der Endkolben in der Haut ausreicht, die dort nachweisbaren Kaltpunkte zu decken, ist eine Frage, die noch zu entscheiden bleibt."[31]

The further we get away from Von Frey the more dogmatic in general become the statements regarding his correlation. The writers of textbooks during the first decade following the formulation of the theory were reserved in their statements concerning it, in the next decade they were less so, and during the past decade the writers have very frequently given the correlation as established fact.[32]

Since we found no specialized end-organs, but only undifferentiated nerve-endings in the serial sections, our results would seem to indicate that the free nerve-endings besides functioning in pain also subserve the functions that Von Frey ascribed to Krause's end-bulbs and Ruffini's cylinders.

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[28]*Op. cit.*, 182. [29]*Ibid.*, 184.

[30]*Ibid.*, 182. [31]*Ibid.*

[32]Lack of space forbids that I list all who sin in this respect, and fairness forbids that I single out any group. Instances, however, are so numerous that citations are unnecessary!

# A BIBLIOGRAPHY OF THE WRITINGS OF MARGARET FLOY WASHBURN

1894-1927

By HELEN K. MULL, Sweet Briar College

## I. BOOKS

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(1) Coöperating Editor. *The American Journal of Psychology.* From January 1903 (Vol. 14, No. 1) to October 1925 (Vol. 36, No. 4).

(2) Coöperating Editor. *The Psychological Bulletin.* From January 15, 1909 (Vol. 6, No. 1) through December 15, 1915 (Vol. 12, No. 12).

(3) Editor. *The Psychological Bulletin.* 7, 1910, August; 8, 1911, August; 9, 1912, August; 10, 1913, August.

(4) Editorial Board. *The Journal of Animal Behavior.* From January-February 1911 (Vol. 1, No. 1) through November-December 1917 (Vol. 7, No. 6. Last Number).

(5) Advisory Editor. *The Psychological Review.* From January 1916 (Vol. 23, No. 1) to the present.

(6) Associate Editor. *The Journal of Comparative Psychology.* From February 1921 (Vol. 1, No. 1) to the present.

(7) Editor. (With K. M. Dallenbach, M. Bentley and E. G. Boring). *The American Journal of Psychology.* From January 1926 (Vol. 37, No. 1) to the present.

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